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THE PSYCHOLOGY OF TESTIMONY *

BY WILLIAM STERN

PSYCHOLOGY claims to be both a theoretical science and at the same time an applied discipline, able to serve in the practical affairs of the world. One such practical problem is that of *testimony (Aussage),* a field in which psychology is able not only to demonstrate its importance in everyday matters but to do so very impressively. It is all the more astounding, therefore, that, in spite of these capabilities, the "psychology of *Aussage*" should have attained to such different degrees of efficiency in different countries. In Germany the movement began at about the turn of the century, initiated by my own investigations and continued and cultivated by many psychologists, jurists, and physicians. In France, A. Binet contributed important suggestions, while later on Gorphe published a comprehensive book on the subject. In other countries, on the other hand, we meet but scarce and occasional contributions. Even in America, which boasts a flourishing psychology of criminology, scant attention has been paid to the psychology of the witness. So far as I know, Münsterberg's *On the witness stand,* published almost thirty years ago, has remained the only monograph written by a psychologist on the subject in America.

To be sure, some of these experimental procedures for investigating the psychological features of testimony, developed abroad, have been taken over and included in American tests—mostly anonymously. I was glad to discover, for instance, in Healy's set of tests and in Bartlett's book on remembrance, certain methods which were very familiar to me inasmuch as I had first applied them. There are other aspects of the psychology of *Aussage,* however, that are very little known in this country. On the one hand, there is the *general* problem—the interpretation

* This article is based upon the last public lecture given by Professor Stern, to the Open Forum of the Psychologists' League on December 29, 1937. It contains the only available summary in English of his own historic contributions to forensic psychology. The author died in Durham, North Carolina, on March 27, 1938. A brief account of his work on the psychology of testimony in its relation to his many-sided contributions to psychological science will be found in the obituary notice in the *American Journal of Psychology,* 1938, **51**, 770–773.—*Ed.*

of *Aussage* experiments; on the other, the *practical* problem—the application of knowledge gained through experiment to the procedures of the courtroom.

In the first half of my lecture, therefore, I shall try to give a survey of the development of experimental methods, a kind of brief history of the psychology of testimony. In the second half I shall attempt to demonstrate the practical importance of these results and the necessity for the collaboration of consulting psychologists as experts in trials. The idea of calling in a psychologist as an expert to evaluate the trustworthiness of testimony is one that is, unfortunately, but little accepted as yet in the United States. In describing to you some of the cases in which I myself or other psychologists have worked as experts, I hope I may do something toward bridging this gap in the international discussion of the psychology of testimony.

EXPERIMENTS ON THE RELIABILITY OF TESTIMONY

Aussage (testimony) means the verbal reproduction of items of *remembrance* referring to a particular experience or event in the past. It is necessarily, therefore, the psychology of remembrance which forms the starting point of these investigations. Indeed, when I began in 1901 to examine the correctness of recollection among my students, I was determined by theoretical interests in the realm of memory rather than by any practical considerations. Yet once confronted with the results, I realized the importance of this research beyond the borders of mere academic psychology. For the crucial outcome demonstrated that a perfectly correct remembrance is not the rule but the exception; that even the most favorable conditions for witnessing and remembering fail to protect people against illusions of a more or less serious kind.

Permit me to illustrate from these half-forgotten pioneer experiments. In one of my earliest, I presented to my students certain pictures, the content of which was to be reported upon after a leisurely period of study lasting forty-five seconds. The subjects knew, while they were viewing the pictures, that they would be required to report on the content, and the report was called for immediately after the presentation. In the reports themselves the subjects were asked to include only those pictorial

items which they still remembered with certainty. There was on the part of the subjects a strong determination to give correct reports; both excitement and emotional inhibition were lacking. Notwithstanding all this, the reports contained on the average 5½ per cent of incorrect statements—nor did the errors always pertain to non-essentials.

Some weeks later the subjects were again asked to describe the pictures from memory—i.e., without seeing them a second time. Though these second reports were only slightly less extensive than the first, the amount of error had doubled (from 5 per cent to 10 per cent). Apparently the long time interval had not damaged so much the quantity as the quality of the testimony; to a considerable degree imagination and confusion had crept into the remnants of former impressions.

After still another interval a final test was undertaken, but with a certain modification. Since it is doubtful whether the subjects would have applied that maximum of care and consideration which are called for from the serious witness in the court room, an attempt was made to strengthen their conscientiousness. When the reports had been written, therefore, I suggested a more rigorous selection of whatever items were remembered with complete certainty by saying, "Please underline those passages of your description upon which you would take your oath before a tribunal." The result was surprising. There were some subjects, to be sure, who narrowed down the extent of their reports markedly and by omitting erroneous passages improved the quality of their testimony. But such procedure was not at all typical. There were many subjects who felt so certain of their report that they were willing to confirm practically all details by oath—the incorrect with the correct. There were, on the average, one or two "perjuries" committed in each individual's description.

An example will illustrate this point. One of the pictures, which represented the household moving of a painter, was described three weeks later as follows by a young man by no means the poorest among the subjects. All the items in italics he was ready to confirm by oath.

The painter is moving. He goes ahead (in the right lower corner). He carries a felt hat. Under his right arm he is carrying some big portfolios, under his left arm a big easel, a palette and a long brush. *Behind him, in*

the middle ground, a four-wheeled cart drawn by a white horse *carries his belongings. In the front of the cart a girl is sitting. A big packing box serves as a seat. She has the reins in her hand. In her left arm there lies a statue representing a tall, nude* youth; in her right arm she holds *a stuffed bird with the wings spread. Behind the girl are some mattresses standing on the short edges; behind these there are some baskets* with boxes, *brushes and pots. One pot is put just on the end of a stick.*

Even though the reader has before him no copy of this picture by which to check the description, there are certain general tendencies which may with profit be pointed out. There is here, first of all, a tendency to overstate the number of items, an inclination toward exaggeration which is a very common feature of remembrance. Thus in the successive descriptions of another picture there occurred the transformation of three trees first into a grove and later into a veritable forest. In the example quoted above there is a second point worth mentioning: a substitution of items originating in a logical need for completeness. The woman, described as seated on a packing box, was actually sitting on a sofa. The insertion of the packing box was not, however, due to a continuous transformation of the mnemic image; it was due to a need for logical completeness. The subject remembered the seated woman but not the object on which she sat; since she must have been sitting on something, he interpolated an obvious object. Had he at all suspected his packing box of being conjecture, not memory, he would not have confirmed it by oath. On the contrary, he believed it to be a real remembrance. This tendency toward logification—the transformation of the unordered and fragmentary chaos of memory images into an ordered, logically satisfactory, and reportable whole—gives rise to many errors in testimony.

Still another source of delusion arises from the verbal formulation of the testimony itself. Possibility of error comes about here in two ways. (1) When testimony is given repeatedly at different time intervals, the expressions employed in the first description come to be more clearly remembered than the objects or events themselves. In short, with repetition the verbal form gradually becomes *stereotyped*—a sign that in place of pictorial remembrance of objects, verbal remembrance has been introduced. (2) But language is also *ambiguous*. If words alone are reproduced, it may happen that in a second or third deposition they will occasion different mental images. For example, one subject,

in describing the artist's moving, used the poetic phrase, "A painter is approaching the gateway of a new life." A few weeks later the word *gateway* was still firmly lodged in his mind, but its original metaphorical import forgotten. Consequently he wrote, "A moving van is passing through a narrow gateway." Of such a gateway there was no trace in the picture.

By still further experiments along the lines indicated above, we were led to the elaboration of a procedure that has become a kind of standard method in Europe. The crucial point in this procedure is the attachment of a cross-examination to the report. We found that, so long as a witness was able to tell his story in a connected manner (as did the subjects in the first experiment), the danger of his going astray was far less than when he was constrained by cross-examination. A new test demonstrates this fact, to a degree beyond expectation. Here too a picture was used, since the comparison between objective stimuli and items reported is thus simplified.

The picture used shows a German farmer's living room with a few pieces of simple furniture. The farmer and his wife, taking their meal with their son, wear characteristic costumes. In a cradle lies the baby. The objects in the picture are colored obtrusively. The subject was allowed one minute for inspection, immediately after which a coherent report and then the replies to a cross-examination were demanded. For this second part a list of numerous questions was prepared. Most of these questions were neutral, concerning persons and things, actions, qualities, special relations actually present in the picture. Eighteen referred to colors, while twelve leading questions were inserted to suggest non-existent objects or qualities. These latter questions were invariably worded expectantly: "Wasn't there a clothes press in the picture?" or "Didn't the boy have on any shoes?"

This experiment was carried out with many children at the age levels of seven, eleven, and thirteen years, and with boys at the age of about sixteen to eighteen years. Without any exception the reliability of the reports in the first part of the experiment was remarkably better than that of the answers in the second part. Summing up the scores on the coherent report (that is, before cross-examination) for all subjects without regard to age, we found an average of 10 per cent errors. This per-

centage is perhaps not too alarming, although important enough. The average distribution of answers to questions in the cross-examination, however, ran as follows:

35 answers, or 60 per cent, correct
17 answers, or 29 per cent, erroneous
7 answers, or 11 per cent, unspecified ("I do not know")

From these figures one can draw two important conclusions: (1) the reliability of testimony drops from a level of 90 per cent in the coherent report to a level of 60 per cent in the answers to cross-examination; (2) in cross-examination the witness, not having the correct answer in mind, prefers to give an incorrect one rather than admit by the words "I do not know" his inability to answer.

The psychology of the "I do not know" is important to the practician; the phrase may bear several interpretations. "I don't know" may be the escape of the lazy person who does not want to exert himself or of the shy person who does not venture to say aloud what is for him a confused notion. "I don't know" may also, however, be the reply of an independently-minded individual who will not respond when memory images are either confused or totally lacking. Such independence is, unfortunately, a rare phenomenon. To many witnesses a question acts as an imperative: "You must answer." They dare not confess their ignorance. Even when they start with an "I don't know," they may be unable to maintain it against the insistence of an inquirer who repeats his question, hammering it home in persistent phrases.

Here, of course, we come to the central point of the whole complex of problems—suggestion. It was one of the most important conclusions of the *Aussage* experiments that leading questions are capable of exercising a well-nigh fatal power. If the expectant tone of the question is very impressive ("Don't you recall that the man had a stick in his hand?") and if the person being questioned does not possess great independence and resistance, it may well happen that the inquisitor will pack many of his opinions into the witness through questions—many more than through the factual recollections he elicits from him.

In the experiment with the picture of the farmer's room, for example, twelve leading questions were interspersed in the

inquiry of each individual. On the average, three of these were answered in the affirmative; i.e., three non-existent objects were stated as existent. Combining the results of all the subjects, from seven to eighteen years of age, we find that the right answer "no" was given to 59 per cent of the questions; the false answer "yes" to 25 per cent; and the indefinite answer "I don't know" to 16 per cent. The younger children, as would be expected, proved to be much more suggestible than the older, but even in the adults the test elicited again and again high degrees of suggestibility. To grasp fully the significance of these figures one must remember that this situation—an experiment—was without hurry or emotional excitement; that immediately after an attentive observation of the picture it was possible to evoke from the subjects unrestricted affirmation of the existence of large objects (a clothes press, a hanging lamp) which were not in the picture, and this merely by the tone and text of the questions.

The following example, though somewhat extreme, illustrates the point. A twelve-year-old girl examined the picture of the farmer's room. To the leading question, "Was there not a clothes press in the picture?" she answered, "Yes." Here I digressed from the prepared list of questions in order to put to her some further queries about the nature of the non-existent clothes press. On all these points she gave detailed information. "What color was it?" "Brown." "Where was it?" "In the right corner." "With one or two doors?" "Two." "Were the doors open so that you could see the contents?" "Yes, clothes." "What was standing on top of the clothes press?" "A flower pot." Had I cared to continue this chain of questions, I should have been able to elicit from the girl a completely mistaken description of the picture, and I as the questioner should have been guilty of this distorted testimony.

Though this case was exceptional, there was no reason to consider it pathological. Hysteria is, to be sure, often characterized by an inclination toward excessive confabulation and a merging of reality and fiction, but occasionally one can find analogous attitudes in healthy persons, especially during certain periods of life. The prephases and beginnings of puberty constitute such a period. One may find in almost every group of children of this age, especially among girls, one or another whose

emotional disbalance and overactive imagination make of her a bad and even dangerous witness—and this without a conscious inclination toward the telling of lies. Such individuals, unfortunately, have very often a strong suggestive power over friends and classmates; and suddenly within a school class a rumor springs up, is accepted, and spreads with the shocking rapidity of fire or an epidemic.

The problem of rumor we also made the object of experimental research. The simple method of successive repetition from subject to subject was first used by myself and my pupils. Recently Bartlett has made a more comprehensive application of the method in his book called *Remembrance,* which includes many striking illustrations. The point to be emphasized is not only that rumor takes over correct, incorrect, and even unbelievable items with the same lack of discretion, but that the content is changed according to the specific interest, the grade of intellectual apperception, or the emotional response from one narrator to the other. In the process the finer nuances of conjecture or doubt become solidified into downright facts. Thus after a few stations of transfer the original content may not only become unrecognizable but even directly reversed in meaning. In an experiment the change can be followed step by step. A text is read to A, who writes it down afterward from memory; A's report is read to B, whose written deposition is read to C, and so on.

All the experiments described thus far are open to one objection. They are too distant from real life. In real life witnesses have to testify about events and actions that they have experienced—experiences which attract the interest to a much higher degree than do lifeless pictures. Possibly a subject careless and suggestible concerning the indifferent items of a picture would prove an excellent observer and reporter when dealing with real and lively events. This suspicion induced the experimenters to elaborate the so-called reality-experiments, to prepare, as it were, theater scenes, though in such a way that the persons present would not become aware of the fictitious character of the occurrence. Then later they would be called on to testify.

The very first close-to-life experiment of this kind was conducted on my suggestion in 1901. The famous German criminologist, F. v. Liszt, inspired by our earlier experiments with

pictures, wanted to adapt the new method to particular forensic requirements. The following incident was therefore arranged during a meeting of his seminar. Out of a scientific controversy between two of the students there developed a quarrel which increased in vehemence until one of the participants drew a revolver. At this point the professor intervened—and asked for written and oral reports from the witnesses.

Although the witnesses were students of law who should have known the conditions of testifying, none of them was able to give a faultless report. The number of errors for each individual varied from four to twelve. The wrong statements accumulated toward the second half of the occurrence; i.e., toward the more exciting events. This last result is especially important, showing as it does that increasing interest not only did not augment but was in fact detrimental to the exactness of the testimony. The explanation here is that interest in the apparent seriousness of the fight was accompanied by a strong emotional shock. Affective reactions inhibit exact observation and reliable remembrance.

Sometimes, on the other hand, the situation is just the opposite. A witness must testify about an occurrence at which he was present physically but without any interest or attention—a condition, that is, the reverse of excitement and interest. To approximate such a circumstance, another experiment was conducted in my seminar. The meeting was held, as usual, in a seminar room which contained a small library, the books from which could be used only in the room itself; all the students knew that taking books from the room was strictly forbidden. During my lecture a stranger entered the room. He asked my permission to look at a book in the library, spent a short time reading in the room, and then went away *taking the book with him*. The students, in the meantime, had paid no particular attention to the man or his action. A week later they were astonished at being required to give *Aussagen* about the incident and the appearance of the man.

It might be supposed that the subjects would plead distracted attention and make but few statements. Surprisingly enough, however, they avoided such cautious procedure in order to make numerous statements—which turned out to be, of course, largely incorrect. No judge could have assembled from the hodge-podge

of contradictory information any fitting notion of the appearance or dress of the man or of such details as whether he wore glasses or not. The principal question, formulated cautiously and with no attempt at suggestion, was, "What happened to the book he was reading?" It was answered correctly by a small minority; a few others abstained from answering; but the majority declared, "He put it back in the book case." This result contradicts one very common rule concerning the judging of testimony. It is the practice, if there are two groups of witnesses whose statements are opposed to each other, to accept as correct the deposition of the larger group. In our case, however, the jury, acting thus, would have been misled.

One may object to the experiment outlined above on the ground that the event was so unimportant and the witnesses ignorant of the fact that they would have to testify. But is not this the conditions in thousands of cases of witnessing? Say there has been an automobile accident or a scuffle with subsequent fight in a saloon. Witnesses are then required to testify about events antecedent to the crucial one. Did the chauffeur fail to give the proper signal? Which of the two men drew a knife first? Very often bodily presence may go along with complete lack of attention and observation; and the inquirer's insistence—"The event happened just before your eyes; you must have observed it"—is psychologically unsound and often nothing but a misleading suggestion.

The significance of such "reality experiments" is not exhausted by their contribution to scientific understanding. Their training value needs to be especially stressed. So many people whose duty it is to extract testimony from others—judges, lawyers, prosecutors, welfare workers and the like—have no idea at all of what it means to testify, for they have never themselves been under the compulsion to serve as witness. There is no more effective way to learn how memory works, how leading questions influence remembrance, than to be a witness oneself. A reality experiment should belong to the permanent stock of instructional devices in seminars for all kinds of professional inquirers mentioned above.

PRACTICAL APPLICATIONS

Before going into the practical applications of the researches indicated above, I must emphasize one limitation of my report.

The practical applicability of psychological methods does not depend solely upon the scientific results of experimentation; it is dependent also upon the structure of the institution involved. In this case the performance of a witness and the manner in which he must depose his knowledge are closely correlated with the whole make-up of justice and especially with the rules of the court. These institutions deviate considerably in America from those familiar to me in my former activity as expert. For example, a fundamental divergence in procedure involves the part which different members of the tribunal take in questioning the witness. Since the testimony of a witness is in the last analysis intended to contribute to the revelation of the truth, it is the custom in some countries to appoint an impartial person as the most adequate inquirer. In such countries it is the judge who summons the witnesses and conducts the cross-examination. In the United States, on the contrary, witnesses are examined by the defense and the prosecution, in the hope that by such antithetic confrontation a balance in the favor of truth will be secured. These differences in customs lead to different practical applications.

One more reservation I must point out here. The work which I am describing deals not at all with the phenomena of *intentional* falsification—the telling of lies and the committing of perjury. These problems are important, but they are already better known and better guarded against than are the more subtle ones of *unintentional* falsification. In other words, my thesis here is that subjective sincerity does not guarantee objective truthfulness, and whatever practical suggestions can be made will turn upon this assumption.

We turn, then, to practical applications. A beginning has been made here, though much remains to be done. A long time and a hard struggle were necessary before a few court officials and others interested in trial procedure began to realize that psychologists could fill a vital need in evaluating the trustworthiness of evidence. To be sure, the testimony in the majority of cases does not require technical psychological analysis. But there *are* crucial trials—and crucial witnesses, whose depositions may determine the whole future course of life for the defendant. To pick out such cases, to call in consultant psychologists and give them the opportunity to observe and examine witnesses, and finally

to incorporate the psychologists' expert testimony in the ultimate evaluation of the depositions—this is the true psychological wisdom which we must strive to incorporate in responsible representatives of law, education and welfare work around the world.

This is one side of the picture. The psychologist on his part must also learn to change his attitude, must prepare to learn anew when confronted with practical tasks. It is not possible to approach living witnesses and the particular conditions of their testimony with mere farmer's room pictures and figures concerning memory illusions and suggestibility. Even reality experiments offer an artificial simplification compared to the seriousness and complication of actual events. Investigations of this sort are indispensable in opening our eyes to new facts and in providing us with the necessary tools for psychological analysis. But just as the physician, in treating the individual, must go beyond the limits of what he has learned in the physiological laboratory, so the consultant psychologist must have incorporated the methods and theories of his science to such a degree that he can deal in a sovereign way with the individual witness. Sovereignty means above all elasticity. Each trial and each individual present unique features and call for particular procedures. Sometimes it is not so much an experimental as an historical method that must be applied. The psychologist may have to reconstruct the history of a given body of testimony, tracing it back to its first appearance and its true origins.

The trial itself, for instance, is usually not the first time that the witness is called upon to give his report. He has talked with others, he has been exposed to rumors, and, above all, he has been questioned on a number of previous occasions, frequently in a more or less suggestive way. The final transformation of all these experiences into the last deposition may sometimes impress the judge or the jury as entirely spontaneous and reliable, but it is the psychologist's work to reveal its true worth—or lack of worth—by a reduction to origins. For example, I was called in at one time as psychological expert in the case of a male teacher accused of having committed sexual offenses with his girl pupils. The main witness, a girl of fourteen or fifteen, gave the court a very vivid description of the specific activities of the defendant. Months before the girl had been questioned for

the first time by a detective who had undertaken the preliminary inquiries after rumors had arisen in the town. Though the detective was present at the trial, the court was about to dismiss him, since his presence seemed superfluous. In my capacity as psychological expert I intervened and asked permission to put a question to him. "How did you question the girl?" I asked. "Oh," he replied with pride and self-confidence, "when she was at first reluctant to give positive items, I insisted urgently and then asked her, 'Did he not do thus and so (giving specific details)?'" It was almost the exact wording which this man had suggested that the girl reproduced later on in an apparently spontaneous manner before the jury. This fact alone, of course, could not decide the case; testimony by suggestion might not necessarily mean falsification. The authenticity of the girl's report was, however, in this instance shattered to such a degree that the acquittal of the defendant was the result—an acquittal based upon this finding in connection with certain others.

It is not by chance that I have chosen this instance concerned with an adolescent. Children constitute a group of witnesses which is for the consultant psychologist the center of interest and main field of his activity as an expert. Above all, therefore, our expert must be a child psychologist.[1] There are two reasons for this particular emphasis on non-adult witnesses. (1) Though many officials connected with trials may have a fair psychological approach to adults, they lack insight into the mental make-up of children and adolescents, thus failing to an astounding degree in handling them. Even fatherhood is no protection against this failure. (2) A second reason—more important, since it originates in the children themselves—is that the undeveloped or only half-developed mind, much more than that of the mature adult, is exposed to factors that tend to distort and falsify testimony. Unstable self-control, a tendency to use lies as a means of self-protection, accessibility to rumors and suggestions, a leaning toward illusions of perception or memory, lack of understanding of the items to be reported on or the seriousness of the situation—these factors play particularly prominent roles in the testimony of non-adults.

A further complication arises in the case of pubescents when

[1] See my monograph, published in 1926 under the title *Jugendliche Zeugen in Sittlichkeitsprozessen* for a psychological analysis of many cases in which I myself and other German psychologists have served as experts.

the dawning sexual urge begins to stir the young girl or boy. At this stage a state of inner disbalance develops, along with a diminishing regard for factual reality; and the resultant emotional unrest allows an uncontrolled imagination to transform images of desire or fear into apparent remembrances. I have had to deal occasionally with fourteen-year-old witnesses who were called upon to testify concerning a man's behavior toward them as much as four years previously—that is, when they were mere children. It is absurd at the outset to require a description of these remote events, but especially absurd when meanwhile a great metamorphosis has taken place within the witness herself. A girl does not need to be a liar or a pathological pseudologist in order to tinge the scanty remnants of childhood experiences with the exciting dyes of new pubescent emotion—until finally she sincerely believes herself to have been attacked in the manner described.

You must not misunderstand these remarks. It should be neither the concern nor the goal of the psychologist to discredit the trustworthiness of testimony in a general way. There is no justification for such a step, since in general the positive value of depositions considerably outweighs their negative features; and, moreover, justice would collapse if deprived of this, one of its main implements for finding the truth. The psychologist must, of course, direct his attention as much toward the signs favoring the reliability of a testimony as to those tending to throw it in doubt. When in the preceding discussion I have stressed those factors which diminish the value of a testimony, it is because common sense has previously worked with a too primitive alternative which I must refute. This common-sense psychology says: testimony is correct so long as the witness is mentally normal and has the intention of telling the truth. Thus—so runs this common-sense conclusion—deviation from objectivity is due either to intentional lying or to pathological pseudologia.

Psychology reveals, however, that between the two extremes of telling the truth or telling a lie there extends a broad intermediate region. Since the intermediate forms are little known and since interrogators themselves influence just these precarious features of testimony through suggestion, they deserve a specific emphasis.

For all this, the psychologist need not take on a negativistic

attitude from the beginning. He may or may not find reasons for distrust. I remember a trial which demonstrates the contrast of a useful with an unfit witness. In this case two seven-year-old girls were the only witnesses. They had told their mother that a man had committed an unlawful act with them. According to the mother's testimony, both had described his actions in almost identical words; both were said to have recognized the man at the police station.

A few days before the trial I had an opportunity to test them in the psychological laboratory, where they presented very dissimilar aspects. Although both proved to be little responsive, the reactions—meager as they were—of one girl to simple tasks were at least correct and suggestion ineffectual. The other girl, on the contrary, was so distracted and so indifferent that we never succeeded in focussing her attention on the tests or in getting from her *any* meaningful reaction.

During the trial itself I was able to demonstrate this difference to the court by an improvised experiment. Among the spectators attending the trial there was a group of my students, some of whom had been in the psychological laboratory when the girls were examined, but others of whom the girls had not met. I confronted each girl separately with each person, asking, "Do you know this man [lady]?" The first girl, though uttering no word, betrayed her recognition of the students seen before by vehemently nodding her head; confronted with unknown persons, she was as unmoved as a statue. Her sister gave no signs of recognition. Her answer to the question was in each case a shake of the head.

Because of these different attitudes, my statement to the court ran somewhat as follows. The first girl, though rather dull, is able to distinguish correct from incorrect answers; she usually avoids the latter, is not easily accessible to suggestion, and distinguishes correctly between known and unknown persons. There is no reason for suspicion that her spontaneous report to her mother or her recognition of the accused was wrong. In the other twin, however, dullness, lack of attention, and lack of emotional reactivity are present to such a degree that no value can be attached to her testimony. The possibility cannot be overlooked that both her report to her mother and her alleged

recognition of the accused were but mechanical apings of the actions of her twin, who quite evidently is the leader within the couple. The judge, therefore, following my advice, limited his sentence to the crime on the one sister, and took no account of the other's testimony.

The problem of *recognition,* touched upon above, is one that deserves particular attention in everyday practice. It very often happens that the witness has only indistinct memory-images of a person whom he saw but once and then perhaps in a state of fright; and he may be quite unable to describe spontaneously the details of the appearance of such an individual. This very weakness of genuine remembrance provides fertile soil for suggestion. And no influence is stronger than to be confronted with a suspect and asked, "Is this the man you saw on the street?" The effect is strengthened by the concrete presence of the man and the fresh optic image which now overshadows the indistinct memory-image. As a consequence, the answer "yes" is all too likely to slip out.

From the beginning of our work with the psychology of testimony we had pointed to this problem and suggested a procedure to reduce the danger. According to our plan the suspect should be included among a group of other individuals not too divergent from him in age and appearance, and then the witness asked if he knows any of these people. If he now identifies the arrested man with the culprit, his statement has a much higher degree of trustworthiness. Unfortunately such multiple-choice confrontations are applied but infrequently. Mostly the suspect alone is put face to face with the witness.

This problem and the manner in which a psychologist may with ingenuity cope with it are illustrated by a trial in which a pupil of mine, Professor Bondy, was called in as expert. A man, X, had been sentenced for illicit relations with two ten-year-old girls. X had throughout protested his innocence, but the girls had recognized him when confronted with him at the police station—even though the identification had proceeded at first "with some hesitation." X appealed to a higher court, but he was again identified by the girls, and the judge was about to accept the testimony. At this critical moment, however, the tribunal learned that in the same region of the city another man,

Y, had committed similar crimes, for which he had been put in jail. A new trial was fixed. X again asserted his innocence. Y entered, dressed in a brown overcoat, but neither of the girls recognized him. At this point Professor Bondy arranged a little test. Since one of the girls had testified that the criminal had worn a black overcoat, Y was ordered in the absence of the girls to put on the black overcoat of another man and to change his seat. One of the girls was then introduced. Asked whether the criminal was in the room, she looked around, pointed at Y, and announced, "There he sits." The same test with the other girl was negative, but meanwhile Y, shaken by the recognition of the first child, unexpectedly confessed.

There are still other problems which the consultant psychologist must face. Occasionally he is asked to help on a case where there is no direct contact with the witness, where his material consists solely of indirect indices—records of preceding inquiries, letters, and so on. In that event his procedure must resemble more and more that of the historian who has to scrutinize documents in order to judge the trustworthiness of memoirs or descriptions of political events. It is precarious, to be sure, to apply this indirect method exclusively; but there *are* occasions —when the crucial witness is dead or inaccessible—when one must try to get along with it.

I was once called in, for example, to give an opinion concerning a trial before a court of labor relations. The question was whether an insurance company had been lawful in firing the chief manager of a branch office without notice. The man had been dismissed originally in a legal way, with salary for three months in advance; but his farewell speech to the employees of the branch office had induced the company to cut this last payment. The farewell meeting took place at midnight after a day of feverish activity. It was attended by a few representatives of the company and by most of the employees, who were deeply moved by the words of their former chief. In the speech, however, there was one word analogous to *rascal* or *scoundrel;* but whether it was used to characterize the trustees of the company or in some quite different context—this problem divided the listeners into opposite camps. The emissaries of the company, who believed the word to have been used with offensive mean-

ing, tried to induce others present to sign a written statement to that effect. But most of the employees gave the word a harmless interpretation.

Since the witnesses did not live in the city where the court and I myself were located, the presiding judge asked me to restrict my analysis to the protocols and to give my opinion as a psychologist. Proceeding with the utmost caution and reserve, I attempted to consider how the mental make-up of both groups of witnesses must have been influenced by the extraordinary situation and by emotional biases. I also pointed out the fact—well known to psycholosists but not to all laymen—that even perceiving (in this case listening to words and grasping their meaning) is determined by the total attitude of the individual. My conjecture was that, under the given conditions, the trustworthiness of the representatives of the company was lower than that of the other group, who, though testifying against the interest of their employers, clung to the harmless interpretation. The judge accepted this analysis and induced the company to pay an appropriate compensation.

Ladies and gentlemen, my lecture has come to an end. I am sorry that I cannot use its content as a little reality experiment. Suppose that tomorrow you had to testify before a court on what I have said, what illustrations I used and in what order, what the elevator boy looked like, etc. Such examination, I am sure, would yield striking contributions to the psychology of testimony. And when the examiner put to you the leading question, "Dr. Stern's lecture was a very boring one, was it not?" let me hope that your affirmative answer would be merely the result of the suggestive power of the question.

PERSONALITY FACTORS D, R, T, AND A*

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IN a previous study the writers attempted to determine by the methods of factor analysis some of the primary dimensions of personality that are indicated by responses to questions in a typical inventory of introversion-extroversion.[1] Three independent factors were rather clearly revealed by the analysis, factor S (shyness or seclusiveness), factor E (emotionality), and factor M (masculinity or dominance). Two additional factors were found to be present, but their exact nature could not be determined. One seemed to be a carefree or happy-go-lucky disposition, for which the term *rhathymia* was coined.[2] The other unknown was tentatively described as "intellectual leadership."

An earlier logical analysis of definitions and tests of introversion-extroversion had led us to expect in addition to the shyness and emotional factors, a thinking factor.[3] The dimension of "thinking introversion," as it was called, would have at one extreme the individuals who habitually stop to think and like thought activity, and at the other extreme individuals who habitually act more promptly and who find thinking less to their liking. Most tests and questionnaires in the past, with the possible exception of Conklin's test,[4] have failed to stress this difference, although it seems more important in Jung's original conception of introversion-extroversion than the social and emotional aspects.

* This is one of a series of articles in which we are indebted to the Social Science Research Council for financial assistance in the form of a grant-in-aid. We wish to express our deep thanks to the Council.

[1] J. P. Guilford and R. B. Guilford. Personality factors S, E, and M, and their measurement. *J. Psychol.*, 1936, **2**, 109–127.

[2] We are indebted to Dean C. H. Oldfather, University of Nebraska, for suggesting this term from the Greek.

[3] J. P. Guilford and K. W. Braly. Extroversion and introversion. *Psychol. Bull.*, 1930, **27**, 96–107.

[4] E. S. Conklin. The determination of normal extrovert-introvert interest differences. *Ped. Sem.*, 1927, **34**, 27–37.

THE INVENTORY

Because of this situation, we set up a new questionnaire including items that emphasize the happy-go-lucky characteristics of people, and also the hypothetical thinking dimension. In connection with the former, we assumed that opposite characteristics, such as depression and having the "blues," would lie at the opposite end of the continuum from rhathymia, and so items of that kind were included. The fact that Mosier had very recently discovered a depression factor encouraged us at an advanced stage of our investigation to find our carefree items at the negative pole of the depression dimension.[5] Items referring to thought quizzed the subjects about their habits of meditation, introspection, and liking for thinking as well as about their habitual alertness to surroundings. Conklin has urged that attention or alertness externally determined is the key to Jung's conception of extroversion.[6]

Items known to indicate the seclusiveness factor were included, first because we wanted some items of known composition in order to see whether there might be any connection between them and the new factors to be discovered; and, second, to test the factor-analysis approach by seeing whether the items would come out with factor loadings comparable with those when the same items appeared in a different battery. This proved to be a happy decision, for it not only vindicated the method, but it also brought to light interesting correlations among the factors. Items found loaded previously with the emotional factor were intentionally left out so as not to introduce too many dimensions. This omission was probably not so fortunate, but it was not at all fatal to the results.

Altogether, 89 items were prepared.[7] Only 30 of them, which are listed below, were factor-analyzed. Two additional items were correlated with all the 30, but the correlations were all so low as to make their inclusion in the analysis of no particular value.

[5] C. I. Mosier. A factor analysis of certain neurotic symptoms. *Psychometrika*, 1937, 2, 263–286.

[6] C. G. Jung. *Psychological types* (tr. by H. G. Baynes). New York: Harcourt, Brace and Company, 1920.

[7] We are indebted to Mr. Robert C. Hall, who aided materially in the preparation of this new questionnaire. It was used in a study reported by J. P. Guilford and R. C. Hall, entitled: The patellar reflex and personality. This JOURNAL, 1937, 32, 275–287.

ITEMS INCLUDED IN THE FACTOR ANALYSIS

1. Are you ordinarily a carefree individual?
2. Do you usually have difficulty in starting a conversation with strangers?
4. Do you prefer to read about a thing rather than to experience it?
6. Do you hesitate to lend your personal property even to close friends?
7. Are you inclined to be considerate of other people's feelings?
10. Are you relatively unconcerned about what others think of your actions?
12. Are you inclined to analyze the motives of others?
13. Do you consider yourself a practical individual rather than one who theorizes?
14. Do you usually keep in close touch with things going on around you?
15. Are you inclined to worry over possible misfortunes?
16. Do you often have the "blues"?
18. Are you inclined to keep in the background on social occasions?
19. Are you more interested in athletics than in intellectual things?
25. Would you rate yourself as an impulsive individual?
26. Do you enjoy getting acquainted with most people?
28. Do you frequently find yourself in a meditative state?
29. Are you inclined to be over-conscientious?
30. Do you often crave excitement?
32. Are you inclined to ponder over your past?
40. Are you inclined to stop and think things over before acting?
45. Are you less attentive than most individuals to things going on around you?
61. Do you like to discuss the more serious questions of life with your friends?
62. Do you like to try your wits in solving puzzles?
67. Would you rate yourself as a happy-go-lucky individual?
68. Do you enjoy thinking out complicated problems?
70. Are you inclined to be introspective, that is, to analyze yourself?
81. Are you usually unconcerned about the future?
84. Do you usually become so absorbed in watching an athletic contest that you completely forget yourself?
85. Can you relax yourself easily when sitting or lying down?
87. Are you more alert to your immediate surroundings than the average person?

THE FACTOR ANALYSIS

The list of 89 items was administered to a thousand students including 610 men and 390 women from classes in elementary psychology where the questionnaire was introduced as a class demonstration. As an extra inducement, they were told that they might know their scores on two traits, "rhathymia" and "thinking introversion" for which *a priori* scoring weights were set up. The 89th item—"Are you a male?"—was correlated with each of the 30 items used in the factor analysis. The resulting correlations were so low that the influence of sex difference can be regarded as negligible. Three alternative replies to the items were permitted: "Yes," "?" or "No."

The frequency tables set up for correlation, however, were reduced to four categories by combining the " ? " responses with "Yes" or "No," whichever made the more even division. Tetrachoric coefficients of correlation were computed by the use of Thurstone's diagrams.[8] These coefficients ranged from —.72 to +.78, with the great majority of them close to zero. They are given, correct to two decimal places, in Table 1.

The factor analysis employed Thurstone's centroid method, and nine centroid factors were extracted.[9] When N equals 1000 and when there is a division of frequencies near the medians in both variables, it was estimated that a significant coefficient would be .10.[10] The extraction of factors was continued until every residual was smaller than .10. It has been our previous experience that it pays to go this far, even though some of the factors prove to be meaningless, for the process is useful in the rotations and serves to bring the final position of the axes into a simple structure. The centroid factor loadings are not presented here, in order to conserve space. They are on file, however, and may be inspected upon request.

The factor loadings after rotation of the axes are given in Table 2, with the communalities of the items. The reliabilities of the items are unknown and so, also, are the specificities. For our purposes here these unknowns are unimportant. It will be noticed that only 12 of the 30 items analyzed have communalities greater than .500, which means that most of the items are very complex indicators, reflecting factors not brought out among the nine of this analysis. Rarely, in our experience, does the reliability of an item such as these fall as low as .500.

Interpretations of the Factors

The psychological problem is, of course, to interpret the factors. Do they seem to coincide with anything real psychologically? Thus far we have merely some statistical constructs which, to be sure, follow from some rigorous definitions and

[8] L. L. Thurstone, and others. *Computing diagrams for the tetrachoric correlation coefficients.* Chicago: University of Chicago Bookstore, 1933.

[9] L. L. Thurstone. *The vectors of mind.* Chicago: University of Chicago Press, 1935.

[10] This value of .10 is about two times the standard error of a tetrachoric coefficient when r equals zero. According to Fisher's table (see J. P. Guilford, *Psychometric methods,* 1936, p. 549), a coefficient about two times σ_r could occur by chance only five times in a hundred when N equals 1000.

TABLE 1

INTERCORRELATIONS AMONG THIRTY QUESTIONNAIRE ITEMS*

ITEM	—1	2	4	6	7	—10	12	—13	—14	15	16	18	—19	—25	—26	28	29	—30	32	40	45	61	—62	—67	68	70	—81	—84	—85	ITEM
2	07																													2
4	10	14																												4
6	23	08	03																											6
7	16	—02	—05	—12																										7
—10	25	04	05	07	19																									—10
12	27	—02	—18	02	12	04																								12
—13	06	08	16	16	—04	08	06																							—13
—14	03	25	16	12	—10	—07	—02	18																						—14
15	40	15	16	11	—25	33	21	15	08																					15
16	35	30	14	16	—06	12	13	23	20	48																				16
18	17	54	24	09	—02	—01	—11	15	34	14	22																			18
—19	24	—06	14	11	08	09	15	10	02	07	07	—04																		—19
—25	30	15	10	14	12	10	—06	—15	—06	02	03	22	12																	—25
—26	09	46	22	10	—25	—09	02	22	32	17	30	44	02	07																—26
28	30	11	08	10	06	12	33	30	17	32	45	14	25	03	30															28
29	39	25	15	07	—03	16	17	06	—03	10	35	21	14	03	12	24														29
—30	27	01	18	02	06	04	—08	—01	03	—02	—05	19	26	14	18	—07	10													—30
32	22	14	11	01	—04	08	18	26	18	39	39	22	04	—14	26	44	22													32
40	38	08	10	09	35	18	14	—14	—18	08	01	13	18	53	05	14	16	09												40
45	08	30	16	10	—20	—06	—08	20	72	19	26	38	—07	—08	30	15	09	17	04											45
61	20	—09	—01	09	08	14	22	06	—10	16	14	—02	14	03	06	34	12	08	16	—12										61
—62	13	03	12	09	05	05	01	06	20	09	18	13	—02	—02	16	08	14	—10	10	06	16									—62
—67	28	02	26	10	35	27	16	—03	08	20	17	15	26	14	20	19	28	—06	15	—06	12	04								—67
68	10	04	08	08	04	—10	02	02	—03	—01	—04	04	10	04	14	17	11	17	04	—08	10	11	25							68
70	30	—01	02	05	06	15	50	14	09	22	29	04	02	06	07	06	09	00	02	40	15	02	21	—02						70
—81	32	—04	09	04	08	35	05	02	00	20	10	08	14	04	02	12	14	04	20	12	04	08	32	22	19					—81
—84	10	10	23	13	03	08	—04	16	18	02	08	25	25	07	20	—02	05	14	12	10	12	11	22	21	05	14	08			—84
—85	35	10	10	07	—11	01	10	12	12	26	38	07	10	—04	14	17	24	04	24	—17	10	16	16	11	—05	16	11	09		—85
—87	—09	19	04	—01	—05	—30	—09	00	35	—02	—04	08	02	06	07	06	—09	00	—02	—08	40	—15	02	—02	—14	—12	—02	08	04	—87

* All decimal points have been omitted. Items with a negative sign before them have been reflected; that is, the coefficients involving those items have had algebraic signs changed.

manipulations. We have nine axes in an orthogonal system representing nine independent dimensions of individual differences in response to these 30 items. Let us see what psycho-

TABLE 2

FACTOR LOADINGS AND COMMUNALITIES FOR THE NINE FACTORS AFTER ROTATION OF AXES TO MAXIMIZE THE NUMBER OF ZERO LOADINGS

ITEM	I	II	III	IV	V	VI	VII	VIII	IX	COM-MUNALITY
1	—.55	.64	.16	.05	.09	.02	—.10	.08	—.20	.806
2	.22	.07	.06	—.65	.04	.02	—.14	—.03	.13	.525
4	.05	—.18	—.04	—.31	—.21	.02	—.11	.22	—.18	.270
6	.21	—.14	.13	—.06	.07	.10	—.08	.13	.06	.131
7	.13	—.19	.28	.20	—.04	—.08	.24	.05	.12	.251
10	—.29	.26	.20	—.06	.01	—.06	—.20	.00	.34	.350
12	.33	—.01	.06	.16	.05	—.48	.12	—.17	.08	.425
13	—.21	—.12	.02	.04	.25	.21	.18	—.22	.15	.271
14	—.12	.01	—.01	.17	.20	.04	.78	.09	—.04	.702
15	.63	—.10	—.13	—.03	—.06	.05	—.08	.06	—.16	.471
16	.66	.06	—.15	—.22	—.02	—.06	—.08	.17	.02	.551
18	.15	—.08	.00	—.69	—.09	—.04	—.21	—.06	—.04	.565
19	—.04	.40	—.11	—.04	.06	.25	—.01	—.35	.00	.358
25	—.04	.47	—.05	.28	—.38	—.09	—.05	.04	.01	.459
26	—.26	—.06	—.15	.55	.24	.08	.19	.00	.04	.503
28	.57	—.02	.07	—.02	.11	—.47	—.14	.13	—.03	.608
29	.29	—.20	—.13	—.24	—.14	—.16	.17	.02	.27	.345
30	.11	.40	—.01	.21	.32	.06	—.05	.05	—.04	.336
32	.53	.10	—.05	—.09	—.26	—.22	—.13	—.01	—.13	.447
40	.10	—.55	.16	—.24	.35	—.17	.19	—.15	—.22	.658
45	.19	.06	—.09	—.23	—.10	—.08	—.75	—.07	.09	.688
61	.24	—.12	—.04	.12	.10	—.39	.07	.13	—.04	.275
62	—.06	—.02	.61	.17	.09	.09	.11	—.02	.06	.440
67	—.30	.76	.20	.06	.19	.07	—.03	.09	—.03	.768
68	.06	—.20	.64	—.04	—.07	—.29	.18	.03	.15	.603
70	.41	—.12	—.01	.07	.04	—.57	—.01	—.23	—.08	.572
81	—.34	.27	.03	—.14	.08	—.08	—.01	.02	.01	.228
84	.06	.27	.02	.22	.24	.15	.16	—.25	.16	.318
85	—.36	.00	.28	.06	.18	.09	.02	—.25	—.32	.417
87	.12	.02	—.01	.09	—.10	—.06	.54	.00	—.22	.372

logical sense they may make. For each factor in turn will be listed the characteristics (as described by the items) of that factor, given in order of the size of the factor loadings. All algebraic signs are given as positive and the characteristics are stated accordingly. Items are numbered so that reference may be made directly to the complete statements previously given.

FACTOR I

ITEM	LOADING	CHARACTERISTIC
16	.66	Often has the "blues."
15	.63	Worries over possible misfortunes.
28	.57	Frequently in a meditative state.
1	.55	Not carefree.
32	.53	Ponders over his past.
70	.41	Introspective; analyzes himself.
85	.36	Cannot relax easily when lying or sitting down.
81	.34	Not unconcerned about the future.
12	.33	Analyzes the motives of others.
67	.30	Not happy-go-lucky.
29	.29	Over-conscientious.
10	.29	Not unconcerned about what others think of his actions.
26	.26	Does not enjoy getting acquainted.
61	.24	Likes to discuss serious questions with friends.
2	.22	Finds it difficult to start conversations with strangers.
6	.21	Hesitates to lend personal property even to friends.
13	.21	Not practical; rather a theorizer.

Items with factor loadings greater than .20 are listed, although it is not possible to say that all of them are significant. We have as yet no way of finding a standard error of a factor loading. The general picture in the list of items loaded with Factor I is that of depression. But it should be noted that although the characteristic "not carefree" has a loading as high as .55, the companion characteristic "not happy-go-lucky" has a loading of only .30. Since we shall find these two items going closely together with high factor loadings in Factor II, we must conclude that the proposed trait of rhathymia is not at the opposite pole from depression. Among items more heavily loaded with the depression factor are those indicating meditation, retrospection, and introspection. Factor D (depression) therefore seems to have a strong aspect of thinking of a meditative sort.

FACTOR II

ITEM	LOADING	CHARACTERISTIC
67	.76	Happy-go-lucky.
1	.64	Carefree.
40	.55	Does not stop to think things over before acting.
25	.47	Is impulsive.
30	.40	Often craves excitement.
19	.40	Prefers athletics to intellectual things.
84	.27	Becomes absorbed in watching athletic contests.
81	.27	Unconcerned about the future.
10	.26	Unconcerned about what others think of his actions.

The thread of consistency running through Factor II is freedom from care or concern; a lack of serious-mindedness and an impulsiveness. It is what we expected to find as our rhathymia factor. Its independence of the depression factor was the most surprising outcome of this analysis. There is a possibility that this factor should be an impulsive trait, as inferred from the significant loadings in items 25, 30, and 40, and from the fact that items 1 and 67 are loaded negatively with the depression factor and so are not pure indicators of rhathymia.

FACTOR III

ITEM	LOADING	CHARACTERISTIC
68	.64	Enjoys thinking out complicated problems.
62	.61	Likes to try his wits in solving puzzles.
85	.28	Can relax easily while sitting or lying down.
7	.28	Considerate of other people's feelings.

There are only two very significant items for this factor. The two refer rather directly to the same kind of interest, although they are worded quite differently. Without other known characteristics to guide us, we may say tentatively that a liking for thinking best describes it. It may be given the label of LT (liking for thinking). It should be noticed that items referring to actual indulgence in thinking are conspicuous by their absence.

FACTOR IV

ITEM	LOADING	CHARACTERISTIC
18	.69	Stays in the background on social occasions.
2	.65	Has difficulty in starting a conversation with strangers.
26	.55	Does not enjoy getting acquainted with people.
4	.31	Prefers to read about a thing rather than to experience it.
25	.28	Not impulsive.
40	.24	Stops to think things over before acting.
29	.24	Is over-conscientious.
45	.23	Less attentive than most indiviuals to things going on around him.
84	.22	Does not become absorbed in watching an athletic contest.
30	.21	Does not crave excitement.

This is undoubtedly our old Factor S, shyness or seclusiveness. It is noteworthy that the factor loading of .69 for the item, "Stays in the background on social occasions," is very close to

the loading (.66) that we previously found [11] for the same item in a decidedly different setting; and that both are very close to Mosier's value of .67 for the same item in still another battery of items and with other subjects.[12] Such agreements, arrived at independently, are a neat vindication of the validity of the factor-analysis approach with questionnaire items. We should not always expect to find such close agreement, even though the method is valid and appropriately used. Further agreement is shown for the item, "Has difficulty in starting a conversation with strangers," which has a loading of .65 here and a loading

FACTOR V

ITEM	LOADING	CHARACTERISTIC
25	.38	Not impulsive.
40	.35	Stops to think things over before acting.
30	.32	Craves excitement.
32	.26	Does not ponder over his past.
13	.25	Practical rather than a theorizer.
26	.24	Enjoys getting acquainted with most people.
84	.24	Becomes absorbed in watching an athletic contest.
14	.20	Keeps in close touch with things going on around him.

of .57 in Mosier's study. The third item, "Does not enjoy getting acquainted with most people," has a loading of .55 here and a loading of .65 in our previous study. To extend this comparison, the item, "Prefers to read about a thing rather than to experience it," has a loading of .32 here, and .43 in the previous study. Allowing for sampling errors in the data, these small discrepancies are probably not significant.

Factor V has such small representation in many items that it would be very risky to name it at all. What little indication we have in the list is of a cool, positive person who lives very decidedly in the present. It has some definite suggestion of immersion in the present environment from the standpoint of an observer rather than of an actor. More pointed questions might bring out its real nature in another analysis.

VI is obviously a thinking factor but it is probably not just what we had forecast on the basis of our earlier analysis, where the label "intellectual leadership" seemed the best description.

[11] Guilford and Guilford, *op. cit.*, 116.
[12] Mosier, *op. cit.*, 274.

FACTOR VI

ITEM	LOADING	CHARACTERISTIC
70	.57	Introspective; analyzes himself.
28	.47	Often in a meditative state.
12	.48	Analyzes the motives of others.
61	.39	Likes to discuss serious questions with friends.
19	.25	Not more interested in athletics than in intellectual things.
32	.22	Ponders over his past.
13	.21	Not practical; theorizes.

Unfortunately, there were not enough items in common in the two tests. The item, "Not more interested in athletics than in intellectual things," had a loading of .69 in the preceding analysis and only .25 in this one. The item, "Prefers to work things out in his own way," had a loading of .34 previously, but it showed such low correlations with all items in this battery that it was not even analyzed. The item, "Inclined to study the motives of others," had a loading of .42 before, and one of .48 this time, which gives the only basis for saying that the two factors are the same.

The present thinking factor indicates the habitual thinker and comes much nearer the hypothetical "thinking introversion" that we postulated earlier. That there are no very high factor loadings for any of the items may mean that this trait is not a strongly organized one, or that the items are rather ambiguous—that is, are indicators of other factors—or that they do not typify this factor as well as some others that were not used. Recalling that many of these items also indicate the depression factor, it may be the ambiguity of the items that is to blame for the moderate loadings. We shall refer to this as Factor T.

FACTOR VII

ITEM	LOADING	CHARACTERISTIC
14	.78	Keeps in close touch with things going on around him.
45	.75	Not less attentive than most individuals to things going on around him.
87	.54	More alert than the average to his immediate surroundings.
7	.24	Considerate of other people's feelings.
18	.21	Does not remain in the background on social occasions.

The first three items, and the only ones having very significant loadings for this factor, are very similar in meaning. Had only one of them been included in the analysis, the chances are that there would have been one less factor to extract and the trait signified by the item would then appear as a specific. As it is, we might say that the trait is relatively specific in that it is almost entirely confined to three items with very similar meanings. Incidentally, the differences in factor loadings among the three items suggest how factor methods might be applied to the differentiation of shadings of meaning. Items 14 and 45 are

FACTOR VIII

ITEM	LOADING	CHARACTERISTIC
19	.35	More interested in athletics than in intellectual things.
85	.25	Can relax easily when sitting or lying down.
84	.25	Becomes absorbed in watching an athletic contest.
70	.23	Introspective; analyzes himself.
13	.22	Practical rather than a theorizer.

FACTOR IX

ITEM	LOADING	CHARACTERISTIC
10	.34	Not unconcerned about what others think of his actions.
85	.32	Can relax easily when sitting or lying down.
29	.27	Not over-conscientious.
40	.22	Thinks things over before acting.
1	.20	Carefree.

more alike in what they indicate than they are like item 87. We might call Factor VIII an alertness factor (Factor A). Whether it will ever be found except in items specifically implying attentiveness to immediate surroundings remains to be seen. Of all factors thus far discovered, this one comes nearest to Jung's and Conklin's conceptions of introversion-extroversion, but is probably too narrow to fit either.

It would be foolhardy to attempt to name Factors VIII and IX. They are the "tail-end" factors that one nearly always obtains in an extended factor problem. We are not even able to set up any hypothetical traits based upon the limited number of characteristics listed for them. The factor loadings are all near the borderline of significance and some characteristics seem

even contradictory. We shall have to be content with the seven describable primary traits or dimensions that resulted from the analysis.

RELATIONS AMONG THE FACTORS

Thus far we have assumed an orthogonal system, with independent primary traits. The factor loadings given above are in terms of such a system. When we plot the points that represent the items with reference to every pair of orthogonal axes in turn, we find that in three cases an oblique rotation is apparently in order. One is the plot with references to axes I and IV, for Factors D and S. Rotating the two axes independently so as to maximize the number of zero factor loadings, we find that the angular separation between them is about 62 degrees. The correlation between the two factors is then the cosine of 62 degrees, or .485.[13] Shy individuals are somewhat prone to depression and depressed persons are inclined to be shy or seclusive; a very reasonable finding. But it is a finding that could not have been predicted so definitely or assuredly on any other basis than a factor analysis or its equivalent.

Fig. 1 shows the configuration of points representing the 30 items projected on the D—S plane. The orthogonal axes are drawn in dotted lines, and the new oblique axes are drawn solid. One could cling to the orthogonal location of the axes, but to do so would be rather arbitrary. The distribution of points more obviously along the planes of the new axes forces us to make the more reasonable adjustment. If one were to leave the axes at right angles and to set up two scoring keys based upon the items represented in Fig. 1, he would undoubtedly find in the end a positive correlation between the scores for factors S and D. There is a possibility, of course, that the inclusion of other test items in the factor analysis might change the looks of the configuration and cause one to revert to the orthogonal axes, but this event seems rather unlikely.

Another correlation between two primary factors seems to involve Factors D and T (I and VI). In this case, axis I seems to be properly located, but axis VI apparently needs a rotation

[13] For the use of the cosine of angular separation as the coefficient of correlation, see J. P. Guilford, *Psychometric methods*, New York: McGraw-Hill Book Company, 1936, 474.

of about 42 degrees toward axis I, leaving an angular separation of 48 degrees. The correlation between D and T would then be .669, which would be regarded as a considerable degree of relationship. The depressed person is inclined to be a thinker

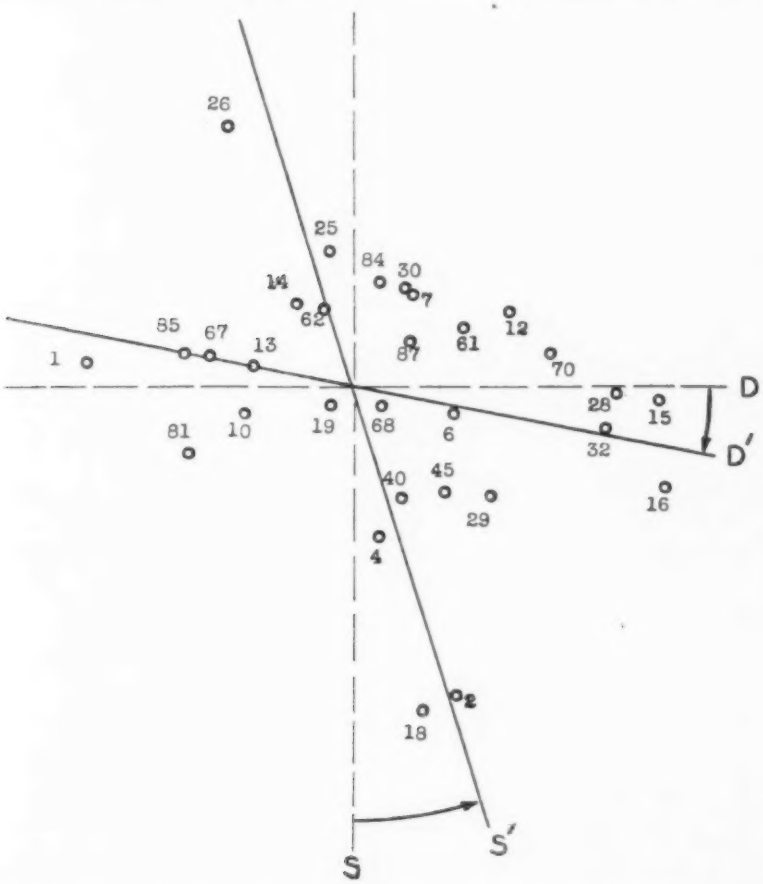

FIG. 1. The 30 test items plotted with reference to the two orthogonal axes D and S, and the location of the two better-fitting oblique axes D′ and S′

or meditator, as we pointed out in discussing the D factor. And the thinker is more likely to be depressed than elated. This finding, too, coincides with common sense. Depressed people are thwarted people, and thwarted people resort more to symbolic modes of reaction. Although the two factors are correlated,

the separateness of them as primary traits is supported by a coefficient of alienation equal to .743.

The involvement of both Factors S and T with depression raises a strong suspicion that there must also be some relation between these two factors. Such is very probably the case. Slight adjustments in both axes IV and VI would bring them within 58 degrees of one another. The correlation between S and T would then be .530. The shy, withdrawing person *is* more prone to thought; and the thinker or meditator *does* find solitude compatible with his bent to reflect and to ruminate.

These relationships have a distinct bearing on the question as to just what is introversion-extroversion. If we do not care to stay by Jung's early definition of that dimension (interest inward versus interest outward), then it would seem that there is some basis for lumping together some characteristics bordering on seclusiveness with some implying a thinking person and still others that indicate depressed emotional tendencies and for calling the resultant picture the introvert. Because of the relationship among these three primary traits it is easy to see how a more cursory inspection of personalities would lead to the conviction of a composite trait like introversion. The opposite composite of sociability, cheerfulness, and lack of meditative thinking would, of course, be the extrovert picture. One would have to omit from the traditional picture, however, some emotional characteristics that do not fit in. The latter, as our previous analysis and that of Mosier have shown, indicate rather the cycloid tendency and emotional sensitivity (Mosier's H factor),[15] and these two factors are shown thus far to be independent of S, D, and T.

The use of the term "introvert" as we have indicated, to represent the person who is simultaneously on the side of the shy, depressed, and thinker, for the dimensions S, D, and T, would then seem to be justified by this statistical analysis. But from this it should not be concluded that the shy person is also *always* depressed or inclined to thought, or that the thinker is necessarily always shy and depressed, or that the depressed person is always a shy thinker. The lack of perfect correlation among these three factors forestalls that conclusion. But it would be

[15] Mosier, *op. cit.*, 272.

true that the stereotyped picture of the shy, thinking, depressed person as an introvert is fulfilled far more often than the laws of chance would lead us to expect, were Factors S, D, and T statistically independent. This undoubtedly accounts for the development of the stereotype. But Jung's intriguing terms were probably unjustly borrowed to fit the stereotype.

It is true that Mosier found his Factors S and D to be independent. He did not find a Factor T because in his selection of items there was no chance for T to appear. How shall we reconcile the lack of correlation between his Factors S and D with our finding? Probably his selection of items did not fill the gap between them but gave an appearance of independence where there is really a relationship. In bringing additional items into the picture it is to be expected that many items that are loaded with the S factor are also indicators of the D factor. Further work will show whether or not this prediction will be borne out.

Since the characteristics "not happy-go-lucky" and "not carefree" have loadings for the depression factor, it might be expected that there should be some negative correlation between D and R. An inspection of the projection of the items in the plane D—R shows some possibility of a correlation between R and D, but it would be relatively small, within the region of −.25 to −.50, with coefficients of alienation within the range from .94 to .75. The relative independence of depression and rhathymia is thus fairly well assured.

One might have expected some correlation between the two thinking factors, Factor T and Factor LT (III and VI). Such is decidedly not the case, if we may depend upon the few items that we found to indicate factor LT. It may be that when additional items are brought into the picture some correlation will make its appearance. One might expect the person who thinks to like that pastime, or one who likes to think to indulge in it frequently. But let us notice the kind of items that are loaded with each factor. Factor T emphasizes meditation, introspection, and retrospection. Factor LT, on the other hand, emphasizes the active solution of problems. Quite different psychological activities are implied by the two sets of items, although both come under the general rubric of "thinking." It is the function or use of the thinking that is important here. One

could like to tackle problems by way of his symbolic methods and yet not indulge in meditation. One could spend much time in introspection and retrospection and little or no time in rational problem solving. It is probably one weakness of the depressed meditator that he will not or cannot solve his own problems in a rational manner; or, what is more pertinent here, that he does not like to do so. From this line of reasoning the statistical independence of Factors L and LT seems justified.

Summary and Conclusions

A set of 89 personality-questionnaire items was prepared with the intention of bringing out more clearly the primary traits or dimensions of rhathymia and of thinking introversion-extroversion that were indicated as probable primaries on the basis of previous thought and investigation. Thirty of the items were intercorrelated, answers having been obtained from a thousand students. Thurstone's method of factor analysis was applied, and nine primary factors were found. Seven of them were identified as: D (depression), R (rhathymia), S (shyness or seclusiveness), T (habitual thinking of a meditative sort), LT (liking for thinking of the problem-solving kind), and A (alertness). The seventh factor was identified with less assurance, but it seemed to suggest an immersion in the present environment as an observer. Significant correlations were found to exist between Factors D, S, and T, which are thought to underlie the common conception of introversion-extroversion. Jung's original conception, however, is much closer to Factor A than to D, S, and T. All other factors seem at present to be statistically independent. The appearance of some correlated primary traits, however, opens the way for others that may well exist to complicate the structure of personality and to confound the direct observer. Evidence was given to validate the use of factor methods in the analysis of questionnaire items. The method holds much promise of future usefulness in untangling the bewildering web of personality.

PROBLEMS AND METHODS OF FUNCTIONAL ANALYSIS IN MENTALLY DEFICIENT CHILDREN *

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Functional Analysis and the Genetic Viewpoint in Child Psychology and in Child Neuropsychiatry

The problems of analysis presented by the mentally deficient child yield only when child psychology and child neuropsychiatry make a common attack. This concurrent operation becomes possible and necessary through an identical point of departure in both fields, viz., the genetic point of view. Let us consider briefly the implication of such a statement.

1. *Child psychology.* Measuring the child's capacity by means of standardized achievement tests has proved to be a most necessary and successful approach in the field of normal child psychology as well as in that of mental deficiency. A true insight into the laws of development and retardation, nevertheless, cannot be gained by this approach alone.

In an earlier paper one of the authors attempted to demonstrate that this approach must be supplemented by an analysis of the mental processes which underlie the achievements.[1] One might think that the growth of achievement parallels that of the functions involved. If this view were correct, one could take the degree of accomplishment as the objective measure of the genetic stage of the functions in question. There is, however, a good deal of evidence that such a straight parallelism does not exist; rather, with the growth in achievement the underlying functional patterns may change fundamentally from step to step. From this latter standpoint functional patterns of

* The authors acknowledge with gratitude the generosity and continued interest of Robert H. Haskell, M.D., Superintendent, and Thorleif G. Hegge, Ph.D., Scientific Director, Wayne County Training School, who have provided the facilities necessary for this study. The authors are also grateful to Ruth T. Melcher, Ph.D., for her reading of the manuscript, and to Dr. Haskell for help in editing the paper.

[1] H. Werner. Process and achievement: a basic problem of education and developmental psychology. *Harvard educ. Rev.*, May, 1937.

different levels are involved in any improvement within a special field of mental activity, no matter whether this improvement is due to training or to maturation. The analysis of these functional patterns, then, becomes a vital task in any field of child development.

Mental development, as defined above, does not mean so much a quantitative increase in accomplishment as a qualitative change in the underlying patterns of functions. For example, genetic psychology makes it seem likely that there is a characteristic transformation of function patterns in the genesis of number ideas during development. First we may find a primary form of the number concept linked up with body activity, motor rhythm, counting by fingers, etc. An important step is taken when numbers are understood as perceptually organized units (especially in the optical field), the so-called concrete number forms. In order to reach a still higher level, another metamorphosis of the process-pattern must occur. The number forms are then stripped of their picture-like properties, and one reaches the level of abstract number concepts.[2]

To illustrate how these different functional levels are interlocked in normal child development we may refer to William Brownell's results in his study on the "Development of children's number ideas in the primary grades."[3] He found that children who could handle numbers adequately on the level of concrete perceptual organization (e.g., as concrete number forms) had little difficulty in making the transition to abstract numbers. If children did not develop mature methods in dealing with concrete number forms—if, for instance, they counted off, point by point, the elements of a point group instead of perceiving them simultaneously as a single unit—those children failed when it came to abstract number operations. Instead, they either carried over their immature method to the abstract sphere, or they attempted blindly to memorize the new material. In other words, they failed in the achievement of a higher functional order because they were already deficient at a lower functional order. These findings in normal genetic psychology

[2] Vide H. Werner. *Einfuehrung in die Entwicklungspsychologie.* Leipzig: Barth, 2nd ed., 1933, pp. 256 ff.

[3] University of Chicago, *Suppl. Educ. Mo.*, 1928, No. 35.

are of a fundamental importance for the understanding of the behavior and functioning of mentally deficient children.

K. Gottschaldt, in his monograph on the structure of the activity of normal and abnormal children,[4] points out that one encounters difficulties if one draws conclusions as to the existence of certain capacities from the objective results in achievement tests. This is particularly true when one tries to compare the performance of normal with that of mentally deficient children. It has been found that children of a lower mental capacity can solve some types of tasks better or more rapidly than individuals intellectually more advanced. In some cases it was possible to analyze the cause. Gottschaldt, for example, presented 8-year-old children with a figure-composition test, the task being to fit together parts made of paper into a simple geometrical figure like a square or oblong. The more intelligent children, having a clear picture of the end figure in mind, were led astray by the natural attempt to conceive the pieces as organic parts of the whole form. It was difficult for them to see those parts mentally as rather artificial and not organic pieces of a main figure. Children of lower intelligence did not have this difficulty, because they were not embarrassed by any discrepancy between a clear and precise image of an end-form and pieces which did not have any organic relation to it. Their idea of the goal was diffuse. They did not care much for the relation of each part to the whole; they merely put the parts together quite mechanically, taking care only that the edges of the different pieces agreed perfectly with each other. By this "chain-like" procedure, characteristic of a genetically lower level, they performed better than most children of the same age but of far superior intellectual capacity.

2. *Child neuropsychiatry.* In contrast to the neuropsychiatry of the adult, child neuropsychiatry must deal with functional disturbances in an organism in which continuous developmental processes of differentiation and integration of functions are taking place. The genetic point of view introducing the factor "time" into the analysis is, therefore, of paramount importance in both fields. In genetic psychology and in child neuropsy-

[4] K. Gottschaldt. Der Aufbau des kindlichen Handelns. *Zsch. angew. Psychol.*, 1933, **68**, 8–9.

chiatry the concept of time came to the fore quite independently, becoming more and more the central aspect.[5]

Child neuropsychiatry, when it began to look for adequate methods, came to realize that it must take over into the field of pathology the approach of genetic psychology and utilize its methods by transforming them according to clinical needs. Functional analysis appears, then, as a task in which the psychological and neuropsychiatric methods of attack are thoroughly integrated.

We shall discuss briefly two of the outstanding problems of child neuropsychiatry which demonstrate the importance of the genetic viewpoint in this field.

(a) One fundamental problem concerns the genetic period at which the organism becomes affected. To illustrate from the pathology of motor behavior: the so-called typical facial expression of a feeble-minded child is scarcely observed if the disturbance occurs after birth. This pathogenetic fact can be interpreted as a sign that mimetic movement, after birth, has reached a stage where it is specifically active, independent of general motor behavior.[6]

(b) The second problem, difficult to differentiate completely from the first, concerns the capacity of compensation for the disturbance with reference to genetic stages. At different stages of mental development the organism's capacity to respond to a defect by some compensatory mechanism differs considerably. At a certain genetic level the organism may possibly be resourceful in finding ways for restitution which are lacking at an earlier stage. Traumata during the first four years frequently lead to general underdevelopment of intelligence. The damage from a trauma of the same kind and extent, when it occurs at a later stage of growth, will usually be more or less repaired.[7]

3. *General procedure of functional analysis.* The general procedure of functional analysis consists of the following steps. (1) One finds, on the level of achievement, a certain deficiency. It may be a difficulty in number operations, or in reading, or in

[5] The "genetic stages" of normal developmental psychology appear in neuropathology as "chronogenous localization" (Monakow).

[6] Vide Alfred Strauss. Beitraege zur Einteilung, Entstehung und Klinik der schwersten Schwachsinnsformen. *Arch. Psychiat.*, 1933, **99**, 693; also his Heilpaedagogik und Klinik, *Zsch. Kinderf.*, 1933, **41**, 445.

[7] Alfred Strauss. *Pedagogia Terapéutica.* Barcelona: Editorial Labor, 1936.

some other category. (2) By various methods one then explores the reactions of the individual in critical situations to discover if there is an impairment of certain functions. (3) When one has found impairments on the functional level, the next step, of course, is to prove that there is a relationship between the deficiency in the particular achievement and the deficiency in the particular function. One may find, for example, an arithmetic disability related to a deficiency in differentiating the fingers. Further analysis, however, may demonstrate that the deficiency is only part of a more "general" functional impairment. It may be, as we shall discuss later, that the disability of articulating the "space of the hand," so to speak, is part of an impairment in the articulation of "space in general."

FUNCTONAL ANALYSES OF INDIVIDUAL CASES

1. *The individual with reference to a population.* A functional analysis should always consider three types of entities. In dealing with a particular deficiency one may start with analysis of a few individuals outstanding in this respect. To evaluate the results, however, one must remember that these individuals, no matter how particular the functional pattern of their disability may be, belong to a larger group characterized by the same disability. In other words, one must evaluate the individual cases, their disabilities and their reactions in the test situations, against two control groups of individuals, the one showing high achievement, the other decidedly retarded. But one should go even further. For any correct evaluation one must consider the standing of the groups within the particular population in which they are embedded.

An important methodological principle is here involved: namely, the principle that the individual should be evaluated in relation to the specific social unit to which he belongs. In the case of any child's activity, the frame of reference should be the structure typical of childhood. In the case of any mentally deficient child one should endeavor to characterize him against the specifically patterned unit of which he is a part.

This consideration becomes significant as soon as deficiency in number work in a generally retarded school population is to be defined. Arithmetic age in its relation to reading age and mental age usually depends upon the outcome of achievement

tests. Since the tests are validated on a normal population, their ratings do not give a fair picture of the achievements for an individual belonging to a particular population. He should be rated in relation to the achievements characteristic of the population of which he is a part. The necessary step then, to get the frame of reference against which the arithmetic and reading ages of the individuals in this study could be rated, was to define the average educational achievement of the total population of the Wayne County Training School in relation to average mental age.[8] The following two tables show these Mental

TABLE 1 *

MENTAL AGE—READING AGE RELATIONSHIP

READING AGE	MENTAL AGE									
	(5)	6	7	8	9	10	11	12	(13)	Total
Below 6		7	9							16
6		2	18	8						28
7	(2)	8	21	24	8					63
8		2	28	35	15	2				82
9		1	10	30	32	17	3			93
10			2	26	48	29	10			115
11			1	8	27	31	20	3	(2)	92
12					6	11	14	3		34
13						1	3	2		6
14								1	(1)	2
15							1			1
16								1		1
Total	(2)	20	89	131	136	91	51	10	(3)	533
Average	(7)	6.4	7.2	8.5	9.7	10.4	11.2	12.5	(12)	

* Mental ages are entered in the columns, in units of one year. Reading ages are entered in rows. The numbers within the table indicate the number of children possessing a reading age corresponding to a certain M.A. In the last row the average reading age has been computed for each M.A.

[8] The population consists of high grade moron and border-line children, with an average I.Q. of approximately 67.

Age-Arithmetic Age and Mental Age-Reading Age relationships.[9]

The tables demonstrate that the achievement in reading is, on the average, .2 to .7 years higher than the corresponding mental ages. The arithmetic age-mental age relationship is quite different. Up to the 9-year M.A. level the average performance is .3 to .4 years higher than the mental age. After the 9-year

TABLE 2

MENTAL AGE—ARITHMETIC AGE RELATIONSHIP

ARITHMETIC AGE	MENTAL AGE									
	(5)	6	7	8	9	10	11	12	(13)	Total
Below 6	(1)	7	10	1						19
6	(1)	5	11	5			1			23
7		4	24	21	3					52
8		3	28	49	19	3				102
9		1	16	36	48	21	5	1	(1)	129
10				15	51	49	26	4	(1)	146
11				4	13	17	15	3	(1)	53
12					1	1	2	2		6
13					1		2			3
Total	(2)	20	89	131	136	91	51	10	(3)	533
Average	(5.5)	6.3	7.3	8.3	9.4	9.9	10.3	10.6	(10.0)	

M.A. level there is an increasing retardation of arithmetic age against M.A. and also, of course, against reading age.[10]

We have stated earlier that the arithmetic ages of these individuals should not be considered in relation to the normal child background, but in relation to the background of the population to which these particular individuals belong. One of our children, having a M.A. of 11 and an arithmetic age of 10.3, cannot,

[9] We are greatly indebted to Mrs. Z. Pauline Hoakley, Director of the Psychology Department in the Wayne County Training School for the work of computing these tables.

[10] The fact that arithmetic age is generally retarded in comparison with reading age, if measured in reference to a normal child population, is not peculiar to our institution. Vide Report by the Committee on Education and Training of the American Association on Mental Deficiency. *Proceedings*, 1937.

therefore, be considered to lag behind in arithmetic, since he actually attains the achievement average for his M.A. To get the numerical expression of this boy's true achievement within his particular M.A. group, one must multiply his arithmetic age of 10.3 by an "adjusting coefficient" *a*. Since $10.3 \times a$ should equal 11, the adjusting coefficient $a = \frac{\text{M.A. (average)}}{\text{A.A. (corresponding average)}}$; *a* for the M.A. level of 11 is therefore 1.07. In the same way an adjusting coefficient *r* has to be computed for reading ages according to $r = \frac{\text{M.A. (average)}}{\text{R.A. (corresponding average)}}$.

2. *The individual with reference to group.* As previously stated, the individuals analyzed must be seen as belonging to one of two *groups* differentiated according to arithmetic disability. In the present investigation, 14 individuals have been chosen as representing the group with excellent achievement in arithmetic, and 14 individuals representing the group with a special arithmetic disability. The two groups were selected according to judgments of the teachers of the Wayne County Training School, judgments which were found to be in high agreement with achievements in the Stanford Achievement Tests. The two groups did not show any differences big enough to impair the results of our experiment. In the "good" group the average M.A. was 8 yrs. 9 mos. (ranging from 7 yrs. to 11 yrs. 8 mos.). The average I.Q. was 66 (with a range from 62 to 78). In the "poor" group the average M.A. was 8 yrs. 6 mos. (from 7 yrs. 3 mos. to 9 yrs. 7 mos.), and the average I.Q. 68 (from 61 to 86). The mean retardation of arithmetic against reading was 14 months in the disability group. (Arithmetic Age and Reading Age were adjusted according to the above-mentioned formulae.) In the "good" group two boys had a reading age 5 months higher than their arithmetic age; the mean arithmetic age of the other twelve was 17 months above the mean of their reading age. Fig. 1 represents the mean relation of Chronological Age, Mental Age, Reading Age and Arithmetic Age in months. Arithmetic Age and Reading Age were adjusted by coefficients *a* and *r*.

A further attempt to express in a single figure arithmetic

achievement in relation to Mental Age and Reading Age leads to a formula first suggested for reading cases by Marion Monroe [11] and simplified later by Hegge, Sears, and Kirk.[12] This formula reads as follows: [13]

$$\text{A.I.} = \frac{2\ \text{A.A.} \times 100}{\text{M.A.} + \text{R.A.}}$$

The Arithmetic Index of adequate achievement equals 100. When we make the necessary adjustments that we have just

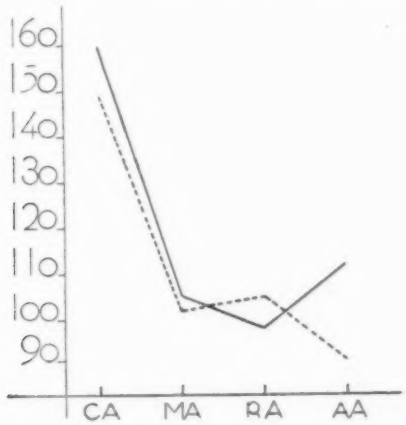

FIG. 1. Black line shows 14 cases of special arithmetic ability (Mean). Dotted line shows 14 cases of special arithmetic disability (Mean).

described, we find in our "poor" group the Arithmetic Index average to be 86 (range from 91–78); in the "good" group the Arithmetic Index average to be 107 (range from 124–88).

3. *Case 1.* This case and the one which follows are intended primarily to serve as illustrations. They should demonstrate how the method of functional analysis might be successfully used in single cases for the uncovering of function patterns underlying a particular deficiency.

The first case to be discussed is that of J. B., a boy 15 years old, with a mental age of 9 years 10 months, Reading Age (adjusted) of 10 years 6 months, and an Arithmetic Age (adjusted) of 8 years 9 months. This boy shows, then, a retarda-

[11] M. Monroe. *Children who cannot read.* Chicago: Chicago University Press, 1932.
[12] Reading cases in an institution for mentally retarded problem children. *Proc. Amer. Ass. Stud. Feeble-minded,* 1932.
[13] A.A. and R.A. adjusted by *a* and *r*.

tion of 21 months in arithmetic as compared with his reading, which was actually superior to his mental age expectancy.

An outstanding peculiarity was revealed during examination: an impairment in the recognition of his fingers and in the use of his fingers for counting. When requested to point, with his eyes closed, to the finger which has just been touched by the examiner, he made frequent errors, especially when the third and fourth fingers were touched. When two fingers were touched successively in each trial, the errors increased. When he was next asked to place his right hand opposite the right hand of the examiner and to indicate on the examiner's hand which of his fingers had been touched, he again made many errors. A picture of a right (or left) hand was then drawn on the blackboard. After the boy's fingers were touched, while his eyes were closed, he was asked to open his eyes and show on the picture which of his fingers had been touched. Here too he failed frequently.

He had difficulty in counting with his fingers. Though he knew instantly the responses up to five fingers, beyond the number 5, where both hands had to be used, he succeeded only by counting one by one and even then he made errors. In brief, we have shown that this boy has difficulty in the differentiation of his fingers, particularly the middle fingers. We may call this a deficiency in the "finger schema."[14]

In order to find out whether a deficiency in the finger schema is related to arithmetic disability, the "Finger Schema Test" was given to the two control groups of high and low achievement in arithmetic. The Finger Schema Test, in its final form, consists of a battery of 13 items. There are 10 trials in each test for the left and for the right hand. The items are as follows:

1. Body orientation: showing on command the left or the right hand, ear, eye, etc. Copying crossed movements of Examiner (e.g., left hand on right ear, etc.).
2. Pointing with open eyes at one finger of the left (right) hand on command: show me finger 1, 3, 5, etc.
3. Same as number 2 with eyes closed.
4. Same as number 3 with 2 fingers called in each trial.
5. E touches one finger of S (eyes closed): S is to point to the finger touched on left (right) hand.
6. Same as number 5 with two fingers touched successively in each trial.

[14] Vide A. Strauss and H. Werner. Deficiency in the finger schema in relation to arithmetic disability (finger agnosia and acalculia). *Amer. J. Orthopsych.*, 1938, 8, 719.

7. E touches one finger of S (eyes closed). S calls the number of the finger touched. Left (right) hand.
8. Same as number 7 with 2 fingers.
9. E's and S's right (left) hand lie opposite each other. S has eyes closed. E touches one of S's fingers. S opens his eyes and indicates on E's hand the finger touched.
10. Same with 2 fingers, successively touched.
11. Right (left) hand picture drawn on the blackboard. E touches one of S's fingers, while S has his eyes closed. S opens his eyes, indicating on the picture the finger touched.
12. Same with 2 fingers.
13. Counting by fingers with closed eyes.

Items 1 and 2 were not included as a part of the test but as a means for introducing the patient into the test situation. Our children, of course, needed repeated instruction to make sure that they understood what the experimenter wanted from them. With our type of children it was also necessary to take into account the state of mind of the examinees in order to evaluate whether the results obtained were pertinent to finger deficiency or to a particular behavior characteristic (rigidity, fatigue, and so on). The time of succession of touching or calling the fingers and the pause between each trial should be such that there is no difficulty in discrimination because of too rapid succession.

The order in which the fingers were called for, or touched, was as follows:

3. Show finger 1 (thumb), 4, 2, 3, 5 (twice).
4. Show fingers 1 and 5, 3 and 2, 4 and 1, 5 and 4, 2 and 4, 4 and 5, 5 and 3, 1 and 4, 4 and 3, 2 and 5.
5. Show finger 1, 3, 4, 2, 5, 2, 5, 2, 4, 3.
6. Successions the same as in number 4.
7. Reverse succession of number 5.
8. Reverse succession of number 4.
9. Show finger 1, 3, 5, 2, 4, 2, 3, 1, 4, 2.
10. Show fingers 1 and 4, 3 and 5, 1 and 4, 4 and 1, 5 and 2, 1 and 5, 4 and 1, 5 and 3, 4 and 2, 5 and 2.
11. Successions the same as number 5.
12. Successions the same as number 4.
13. Show 6 on your fingers 8, 5, 9, 7 (twice).

The errors were counted by points: 1 error with no. 3 was given 3 points; 1 error with nos. 4, 5, 7, 9, 11 was given 2 points; in the other tests the error was counted as 1 point.[15]

[15] The errors have been differentiated according to the difficulty of the test on a scale of three points: an error made on the easiest test is assigned a value of 3, an error on the most difficult test 1, and an error on a test of medium difficulty, 2.

Characteristic differences were found when the finger schema tests were administered to the two groups. In the "good" group, out of 210 possible errors only three children made a few errors, eleven children made no errors at all. In the "poor" group, with the exception of one boy, all children made errors ranging in number from 4 to 66 (in points, from 10 to 100) with an average of 36 errors.

These results suggest a relation between deficiency in the finger schema and deficiency in arithmetic achievement. They support the assumption that, in our study of case 1, the deficiency in the differentiation of the fingers has a definite bearing on his arithmetic retardation. The functional analysis, of course,

TABLE 3

DISTRIBUTION OF ERRORS IN FINGER SCHEMA TEST

	ERRORS										
	0	10	20	30	40	50	60	70	80	90	100
"Good" group	11	3									
"Poor" group	1	1	1	3	2	1	1	2	1		1

should go even farther. The question still remains whether, in the individual case, one can link such a deficiency in the finger schema to a functional impairment of a more general nature. We shall return to this question after having discussed some further peculiarities of the boy, J. B.

On tests of visual perception he was able to copy very simple designs constructed with matches (see Fig. 2, nos. 1, 2, 3), but erred considerably when slightly more complicated figures (such as 4, 5, 6) were used. This task all our children with a mental age of seven could solve correctly. We feel justified, therefore, in contending that this boy, with a Mental Age of almost 10, has a considerable impairment in the ability to perceive spatial directions and to relate forms spatially to one another.

This disability in spatial construction was again demonstrable in test situations of the following nature. A square paper was folded diagonally before the boy's eyes. Requested to draw a

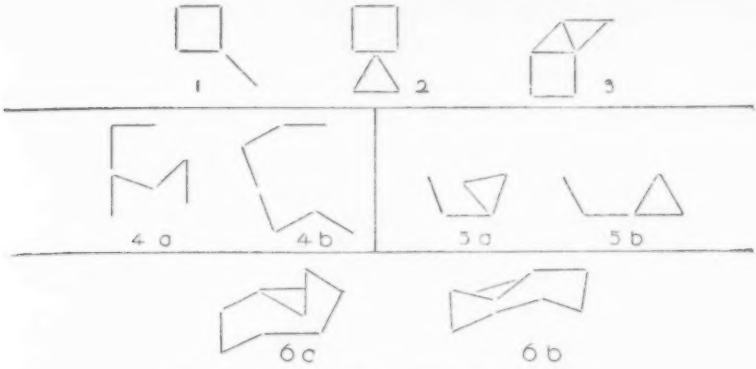

FIG. 2. PATTERNS CONSTRUCTED BY MATCHES
The *a*-patterns are the models; the *b*-patterns the copies.

picture of the paper with the diagonal in it, the boy made a correct drawing. Next he was asked, "If I should cut this paper with scissors along the folding line, how many pieces would I get?" "Two!" "Now draw me a picture of one piece." The boy drew Fig. 3.

FIG. 3. S's DRAWING OF HALF OF THE SQUARE PAPER

The examiner then actually cut the paper apart and, after holding the two pieces closely together, put them out of sight, and asked the boy to make his drawing again. After an incorrect response (1 of Fig. 4) a second and a third demonstration were given. Each time the two pieces were presented farther from each other. It was not until the two pieces were perceptually far apart that the correct drawing (3 of Fig. 4) was made.

FIG. 4. FURTHER DRAWINGS BY S OF THE HALF OF SQUARE PAPER

Another question accompanied by demonstration was: "If I should cut off a small piece at the corner of this paper what would the remaining part of the paper look like?" The boy

drew Fig. 5-1. After being told that the drawing was incorrect he himself invented a means for getting around the difficulty. He drew first a picture of the paper, then marked the piece

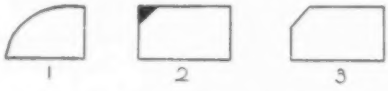

FIG. 5. S's DRAWINGS OF A PAPER WITH THE CORNER CUT OFF

to be taken off. (See Fig. 5-2.) After that procedure he drew the correct figure as in Fig. 5-3.

In still another test the examiner lifted a sheet of paper before the boy and explained: "Imagine that somebody cut a hole into the sheet at this point [E points to the upper left-hand corner]. Now let's turn the sheet around so that the upper edge is at the bottom!" Requested to draw a picture of the sheet in the upside-down position and to indicate the hole, the boy gave the incorrect representation in Fig. 6.

FIG. 6. S's DRAWING OF A PAPER WITH A HOLE IN THE CORNER

All these tests of mental operations in visual space could be performed by our children of seven years of mental age. This fact adds to the evidence for the conclusions advanced above, that this boy is severely impaired in the ability of spatial construction. Tentatively one may venture the assumption that there is a linkage between the boy's impairment in differentiation of the fingers and this disability in comprehending spatial relations. Such an interpretation is supported by some evidence from pathology which would lead too far to be discussed.[16] The assumption of such a linkage is understandable from the point of view that the relation of the fingers to each other and to the whole hand is a particular form of spatial relationship. If such an interpretation could be accepted, functional analysis of arithmetic disability would have revealed in this individual a specific impairment, viz., deficiency in the finger schema related to impairment of a more comprehensive nature—that is, a deficiency in the function of space relation in general.

[16] Vide Johannes Lange. Fingeragnosie und Agraphie. *Monatschr. Psych. Neurol.*, 1930, **76**, 129.

4. *Case 2.* The second principal case is a boy, J. F. His chronological age is 16 years, 7 months, his mental age 13 years, 2 months, his I.Q. 82. His reading age (adjusted) is 13 years, 10 months, his arithmetic age (adjusted) 10 years, 11 months. His retardation in arithmetic against reading amounts to 2 years, 11 months.

In the finger schema test he made relatively few errors [17] and these only in the more complex situation.[18] He did particularly well in all tests which demand a comprehension of concrete visual space relations and an ability for mental operations in visual space, as in Thurstone's Hand and Spatial Relations Test, and others. His performance on the Stenquist Assembly Test was at the adult level. In surprising contrast to the high achievement in tests of this sort, his performance was very low on visual tests when patterns made up by discrete elements, like dots, were presented to him. These facts, brought out first by preliminary experiments, instigated further analysis. Two of the preliminary experiments may be described briefly.

Test No. 1 was a "delayed reaction" experiment. In this test there were 7 sets of 3 patterns each. Each pattern consisted of 2 to 8 dots; the number of dots was the same in each set. Each set was presented simultaneously in one row (exposure: 3 seconds). After a pause of 3 seconds one of the 3 cards was presented again, and the boy was to tell whether this card had been at the left, the middle or the right when presented originally with the two other cards. He was able to give no correct answer in this experiment when more than four dots were involved in a pattern. On the other hand, when ordinary full-line forms were used instead of dot patterns, the limit of correct answers went up: 6-line figures were still recognized correctly.

In Test No. 2, a recognition experiment, 7 sets of 6 cards each were used. For each set the number of dots composing the various patterns was constant; they varied from 2 to 8 in the different sets. The boy was presented with one of the 6 cards first (3 seconds exposure) and was then asked to pick out the identical card from the 6 successively presented patterns. He was correct only as far as a 4-dot pattern. Here again he

[17] Twelve points on our scale. Vide Table 3.
[18] Indicating touched fingers on E's hand and on the hand pictured.

did better when the full-line patterns were used, for he recognized 6-line figures correctly.

These results, preliminary as they may be, provoke further questions. (a) Can we get more conclusive evidence that the deficiency actually lies in the comprehension of patterns made up by discrete elements rather than of the fully integrated line patterns? (b) Is this deficiency specifically limited to the visual field?

To answer the first question, a test situation should be found which increases the difference in the results with the two types of patterns. One way to do so is to increase the difficulty of the task, which can be accomplished by requesting the subject to keep in mind two patterns at a time instead of one (as was done in the recognition experiment). In this experiment cards were used which again contained dot patterns in one series, full-line patterns in the other. The boy was presented with two of these cards successively—instead of one. We may call this experiment, therefore, the "double-pattern" experiment. After a 3-second pause he was to draw the first or second pattern. Here he made errors when 2 dots were used. This is a drop of 2 dots in the "double-pattern" experiment against the recognition experiment mentioned before where only one pattern had to be retained at a time. With line patterns, however, his performance was still relatively high, viz., 5-line elements; it will be remembered that he had scored 6 lines correct in the single-pattern experiment. In view of these results, the conclusion is strengthened that there is here a particular deficiency in the grasp and retention of patterns made up by discrete elements like dots.

The second question, as to the limitations of this deficiency to visual objects alone, may be answered by comparison of S's performance in the visual field with those in other sensory fields. Experiments analogous to the above were consequently carried out in the auditory, tactual and kinaesthetic fields. In order to make the auditory experiment analogous to the dot experiments, no "meaningful" intervals or melodic phrases were used. There were only two pitches, low (520 cycles) and high (534 cycles). The number of tones building up the pattern varied

from two tones to eight. To show how the experiment was conducted we may symbolize graphically one series of tonal patterns, a 5-tone pattern. See Fig. 7. The dots represent the tones in pitch; the time grouping is represented by the distance. The boy was to identify pattern *x* with one of the 6 patterns presented afterwards. He was correct with patterns made up of 6 tones: a relatively high performance.

In a second experiment, analogous to the visual "double-pattern" experiment, two tonal patterns were presented to the boy, who, after a pause of three seconds, was to indicate by voice either the first or the second pattern. In this experiment it became very clear that he was well able to keep in mind these two patterns. He was able easily to shift from one pattern to the other or to correct himself, even after reproduction. He

FIG. 7. EXAMPLE OF THE ARRANGEMENT OF TONES IN THE AUDITORY EXPERIMENT

could never perform operations of this sort with visual patterns, where he became completely confused when any difficulty arose. In accordance with the ease of his auditory performance and in contrast to the visual tests there was only a small drop in this "double-pattern" experiment against the one-pattern recognition experiment: two 5-tone patterns could be remembered and produced.

If differences of intensities, instead of pitches, were used in an otherwise similar tone experiment, he scored even better: a 7-tone pattern made up by two intensities was easily recognized; in the "double-pattern" test 5-tone patterns were still correct.

Again, analogous experiments were devised to test the kinaesthetic-tactual field. Patterns made up by taps of two different intensities varying from two to nine elements were applied to the subject's skin. In the recognition experiment he had to keep in mind one pattern (. . . . .) and to identify it amongst five other patterns. In the "double-pattern" experiment, two such patterns had to be kept in mind and reproduced

by tapping at the request of the examiner. In the one-pattern experiment he scored 7 elements; in the double-pattern experiment, two patterns composed of six elements obviously did not interfere with each other.

From the excellent performances of the auditory and kinaesthetic-tactual experiments the conclusion can be drawn that the deficiency observed in the optical field does not obtain in other sensory fields.

It appeared desirable to introduce a test in the tactual-optical field. Without allowing the boy to see what was being done, a pattern was applied with a needle to the back of his hand. He was then asked to reproduce the pattern on paper. Here he was correct as long as only one simple and impressive figure,

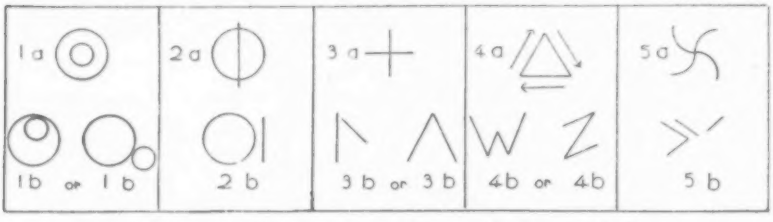

FIG. 8. VISUALIZATION OF TACTUAL FORMS BY J. F.

like a circle or a line, was presented. As soon as forms consisting of two or more discrete elements were introduced, he made significant errors; his performance here was quite different from the performance of our much younger children. He failed, for example, in the cross and in the concentric circles: that is, he drew each element correctly but was unable to integrate those elements into the correct pattern. (See Fig. 8.) This tactual-optical experiment demonstrates that his deficiency, although not noticeable in the purely kinaesthetic-tactual field as shown in the tapping experiment, becomes effective in the tactual sphere as soon as visualizing factors are involved.

To summarize the results of the different tests, it would seem correct to conclude that this boy is deficient in visual imagery of patterns made up of discrete elements. Such a conclusion raises the problem as to whether or not this specific deficiency has a relation to arithmetic disability.

This problem was attacked by a study made of the two groups

mentioned above, one of which showed decided arithmetic ability (the "good" group) and the other deficiency in number work (the "poor" group). Both groups were presented with dot patterns increasing from 3 to 7 dots. Each pattern was exposed—by means of a tachistoscope—for 1/10 of a second, and the children were asked to reproduce the form on paper. There were two tests, A and B: the first consisting of 11 patterns; the second, a more difficult one, of 18 patterns. (Fig. 9.)

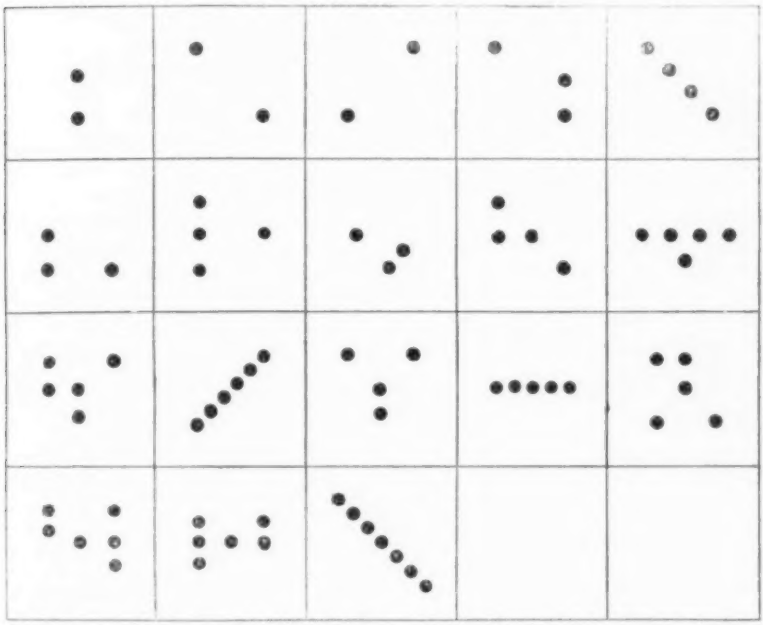

FIG. 9. PATTERNS USED IN TACHISTOSCOPIC EXPERIMENT

The results of these two tests for the two groups, distributed according to mental ages, are presented in the graphs in Fig. 10. In test A, the performance level in the "poor" group was decidedly lower at each mental age than in the "good" group. The same is true for test B. In comparing the results of the two tests as to correctness of the form, it is clear that test B is of a much higher diagnostic value than test A for three reasons. (1) In test B there is no overlapping at any mental age level in the performance of individuals from the two groups.

(2) In test B the difference between the groups is much larger. (3) Whereas the two curves decrease almost parallel with increasing age in test A, in test B the discrepancy between the curves grows with mental age, to indicate probably that a very deeply rooted functional deficiency is involved.

Although the number of children participating in these tests is relatively small, the conclusion seems justified that a distinct relationship exists between the functions involved in the comprehension and retention of visual dot patterns and arithmetic

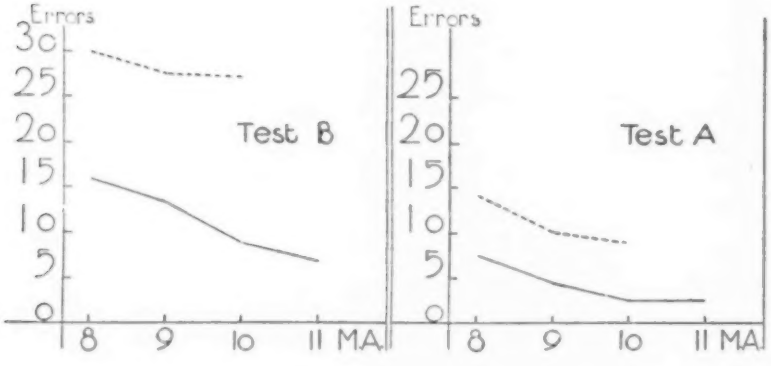

FIG. 10. GRAPHS REPRESENTING RESULTS OF TACHISTOSCOPIC EXPERIMENTS A AND B
Black line: "good" group. Dotted line: "poor" group.

achievement. The score of our case 2, J. F., definitely places him in the disability group. His errors in form reproduction amounted to 25 points. Considering his mental age of 13 years, his score is about what one would expect from a member of the disability group.

Another visual test, originally devised by Laemmermann (Germany), has been given as a group test to the population of 250 children regularly attending the academic classes of the Wayne County Training School, in groups of approximately twenty at a time. The test consists of 6 patterns which the teacher draws one by one into squares on the blackboard.

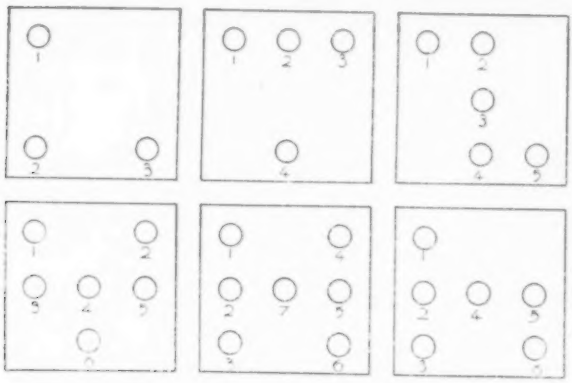

FIG. 11. LAEMMERMANN TEST

The method of administering the test is as follows:
The teacher draws three squares on the blackboard and says:

> On your paper you will find squares such as I am drawing on the blackboard. There is a red dot at the top of the paper. Write your name at the side of the red dot. Now I want you to draw in each square little circles like this (o). I make these circles first on the blackboard and you look at them very carefully. Then I erase them and you will draw the circles in the first square. You have to be careful to copy them exactly as I make them. You have to make the same number of circles, and these circles must be in exactly the same places where I draw them. Don't take the pencil before I say, "Now."

The teacher draws three circles in the first square. The numbers indicate only the order in which the circles are drawn by the teacher and should not appear in the drawing. The teacher turns to the pupil and says:

> "Now look at this picture carefully." (He waits three seconds, erases the circles.) "Now draw. — — — Those who have finished put their pencils aside."

The same procedure is observed with the other two squares. It is important that the pupils do not take the pencil before the command "Now." Furthermore, an interval of three seconds should be kept rather uniformly. After completing the three squares, the pupils turn the paper over for the next three squares and are told: "Write your name again at the side of the red dot. The red dot must be at the top of the paper."

The form scoring has been done in the following way. One point of error: a slight deviation from the pattern or shift into a small section of a square or to the side. Two points of error: form half correct; shift of more than one circle within the figure; addition of more than one circle, etc.—but the form being still recognizable. Four points of error: complete failure. See Fig. 12.

The results with the Laemmermann test are shown on the graph Fig. 13.[19] The average points of errors (Y axis) are

[19] We are greatly indebted to Mrs. Hilda Abrams, teacher at the Wayne County Training School, for the scoring of the group test.

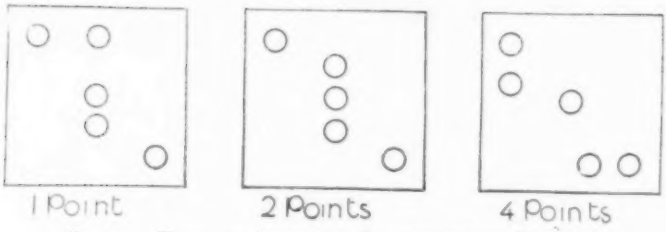

FIG. 12. TYPES OF INCORRECT LAEMMERMANN PATTERN

plotted against the mental-age levels (X axis). The full line indicates the form curve, the dotted line the number curve. There is a constant decrease in form, as well as in number, errors. These results prove that there is a definite relation between the performance in this test and the mental age. In order to determine whether or not there is a relation between that performance and arithmetic, we turn again to the two groups mentioned previously, one of which showed high, the other low achievement in number work. For each child, the deviation of his test score from the average score for his mental age has been defined. A graph, Fig. 14, shows the results of this procedure.

Though there are certain irregularities, on the whole the graph demonstrates a rather clear relationship between accomplishment in the test and arithmetic achievement. In the "poor" group there is only one case that shows slightly fewer errors than the average for this mental age. Among the "good" cases, three children have more errors than the average for their mental age group.

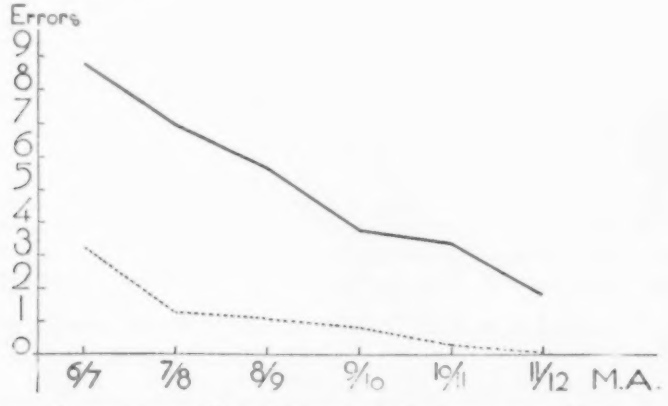

FIG. 13. LAEMMERMANN TEST: RELATION OF PERFORMANCE TO MENTAL AGE

Once more comparing the performance of J. F. with that of the group, we find that he made 8 error points in form, and 2 in number. He scores, therefore, on a 7- to 8-year mental-age level, again remarkably low.

In summary, it has been shown by individual and group analysis that J. F. has a definite functional deficiency. This deficiency is very circumscribed and can be characterized as inability to grasp and retain visual patterns made up by discrete elements. In view of the results from the group tests one has every reason to conclude that this deficiency has a bearing on his disability in number work.

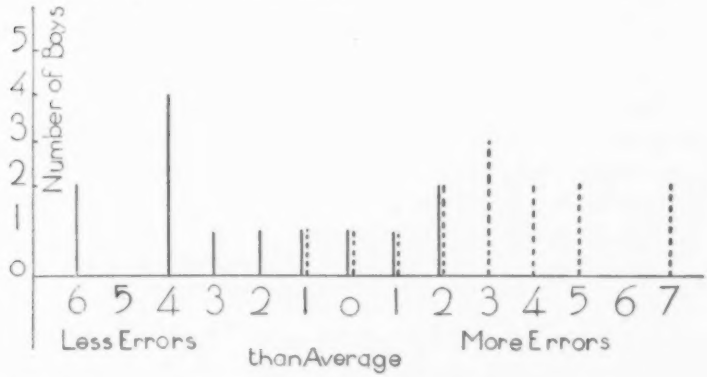

FIG. 14. LAEMMERMANN TEST: RELATION OF PERFORMANCES OF 28 INDIVIDUALS TO THE AVERAGE PERFORMANCES OF THE CORRESPONDING MENTAL AGES
Black lines represent boys of the "good" group; dotted lines, boys of the "poor" group.

The problem remains whether or not functional analysis can go further in interpreting this very circumscribed and specific impairment as being intimately related to a deficiency of a more general nature. Theoretically, such a deficiency could have several quite different sources. For example, it could be caused by a diffuseness in comprehending and discriminating patterns as such; or, by a low ability to localize single elements in space and grasp the spatial relationship between them, and so on. Most of these interpretations are easily ruled out, so far as J. F. is concerned, but there is some evidence for the assumption that the boy's failure in the visual tests is caused by his inability to keep in mind simultaneously several aspects of a visual object. If this assumption is justified, one should

expect him to fail in the visual tests with the dots where he has to pay attention to their number and form on the one hand, and to the shape of the whole pattern on the other.

Some of the data on which that assumption might be based will be discussed briefly.

(1) In one of the experiments the boy was presented (1/10 second exposure) with cards containing differently colored forms. There were from three to twelve of these forms on each card. First, he was asked to tell the colors and the number of figures. He was correct with four elements. In the second series he was requested to neglect the colors and to count the forms only. Here he was correct with nine elements. If we compare his achievement in this test with the achievements of the two groups differentiated according to arithmetic ability, the remarkable result is that, so long as he had to tell only the number of the forms, his achievement did not go below the average of the older boys belonging to the "good" group. As soon as he was required to observe the colors and count simultaneously, his counting ability dropped far down to the youngest mental ages (7 to 8) in the "poor" group. Such a discrepancy seems to establish, at least for this test, an inability to keep in mind two aspects of a visual object at the same time.

(2) Among the dot patterns used in the previous experiments, there were some which consisted of a series of dots like these: • • • •. Under the ordinary procedure, when J. F. did not know what kind of pattern would be exposed next, he scored very low on these patterns, missing even four dots. In contrast to this result, when he was told beforehand that he was about to see lines of dots, he ranked among the best of our children, having more than seven elements correct in his drawings. This discrepancy can be explained on the basis of his supposed disability. When he had to pay attention both to the elements and the configuration as a whole, he broke down. When the configuration was presented beforehand so that he could concentrate on the elements alone, his performance was quite normal.

(3) A similar interpretation may be drawn from the fact that his score was relatively high on patterns formed by outlines, but low on patterns formed by dots.

(4) It will be recalled that his performance in the "double-pattern" test, where he had to keep in mind two forms at the same time, was extremely low. Since this task must be particularly difficult for one who cannot grasp various things at the same time, the results with this test support our interpretation.

(5) Finally the results of the test necessitating visualization of forms drawn upon his skin seem to point to the same conclusion. He was able to recognize simple forms, but he was deficient in grasping several elements and relating those elements to one another in a characteristic unit at the same time.

SUMMARY AND CONCLUSION

In conclusion we may summarize the methods and procedures of functional analysis and point out its practical advantages. Since we are dealing with unknown factors underlying the disability in question, the most successful approach to the problem is the selection of cases that show a striking discrepancy in different achievements. The results of analyses applied to a few carefully selected cases provide us with methods and points of view for investigation of a larger group having a similar type of deficiency.

As we have repeatedly stated, the general method of functional analysis is the examination of an individual in critical situations which elicit the impaired functions. The proof that the functional impairment is essentially related to the particular deficiency rests on two conditions:

(1) One must show that two groups, outstanding for high and low achievement in the performance in question, can be differentiated on the basis of the critical tests derived from analysis of the single cases.

(2) The individual classified as outstanding in his functional impairment should rank low when his test performance is compared with the average of the deficient group.

When functions bearing on the disability in question have been isolated, one must proceed further to evaluate the impairment as *specific* or *general* in nature. The two main cases analyzed in this paper may serve as an illustration of such a procedure.

Case 1, an individual with arithmetic disability, was found to have a specific disturbance in the finger schema. The same disturbance was demonstrated in a group similarly handicapped. Functional analysis of the individual case revealed further that the general ability to grasp relations in space was here disturbed. The assumption seems warranted that, for this particular individual, this general disturbance is underlying the specific impairment viz., the deficiency in the finger schema.

Case 2 illustrates another pattern of impaired function underlying arithmetic disability. Here the specific disturbance in the comprehension of visual patterns is present to a significant degree in the disability group. A more general impairment discovered in this individual, which is probably at the root of his specific disturbance, is the deficiency in grasping simultaneously the multiple aspects of a visual unit.[20]

Finally, it should be emphasized that the results of functional analysis applied to individual cases should be evaluated against the general background of the child's personality, peculiarities in behavior, and other abilities and disabilities.

It is clear that the results of functional analysis, rather than the data from achievement tests, should serve as a guide for remedial work. The methods, techniques and materials for training must be chosen for their adequacy in relation to the functional impairment.

[20] Three points characterize the difference between a general and a specific factor:
(1) The connection of specific disability and specific functional factor is more direct, closer than that between disability and general factor.
(2) The power (and the probability) of evoking a specific disturbance is greater with the specific than with the general factor.
(3) General factors may change from individual to individual each of whom has the same specific disability, while the specific factor may be effective in every one of these cases. This circumstance is in accordance with the clinical experience that there is a greater frequency in the occurrence of the relation, specific factor-specific disability, than in the occurrence of the relation, general factor-specific disability.

THE EFFECTIVENESS OF VARIOUS TYPES OF ART APPRECIATION AIDS *

BY KATE HEVNER AND JOHN H. MUELLER
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Purpose and Method

The purpose of this experiment was to discover the effect of various kinds of information or appreciation aids on the judgment of pictures. The method was to exhibit to 180 subjects, students in Fine Arts and in Psychology at Indiana University, five modern landscape paintings [1] and to ask the subjects to locate each picture on a nine-point rating scale, as follows:

−2	−1	0	+1	+2	+3	+4	+5	+6
Dislike		Neutral; indifferent; would not look twice		Fairly pleasant; would not care to own		Very pleasant; would like to own		Extremely pleasant; would sacrifice to own

When the first impression had been recorded, each subject was provided with four mimeographed pages of material describing four of the five pictures and including one each of the four types of appreciation aids. After studying this material for twenty minutes, the subjects re-rated the pictures, recording their final judgment on these four as well as on the picture for which no material had been provided. A system of rotation had been worked out for these appreciation aids, whereby all possible combinations of the five pictures and four types of appreciation should appear together, and each picture and each type should appear equally often.

The four types of aids were as follows: Type A, anecdotal and biographical data on the painter, and Type B, a literary interpretation of his work. These were taken from current

* Financed by funds from the Graduate School of Indiana University.

[1] The pictures were framed prints about 23″ x 36″ including *House on the Hill* by Paul Cézanne, French, 1839–1906; *Landscape in Southern France* by André Derain, French, 1880– ; *Monhegan on the Coast of Maine* by Rockwell Kent, American, 1882– ; *Summer* by Claude Monet, French, 1840–1926; *Country Road* by Maurice de Vlaminck, Belgian-French, 1876– . These prints were loaned to the experimenters by the Department of Fine Arts of Indiana University.

magazines and books on art appreciation.[2] They were entirely divorced from the particular picture in question and could have been used equally well with any one of the many pictures done by these painters; in fact in Type B they could be used almost indiscriminately with the work of any other painter with no loss in effectiveness. For Type B paragraphs were made in some cases by putting together widely scattered remarks from a variety of authors and chapters. The names of schools and painters were used without definition or explanation. A deliberate effort was made to include obscurities and clichés, platitudes, glamour, critic's patter, meaningless laudatory phrases and literary ornament for its own sake; e.g., "he achieved a unified interplay of plastic elements to obtain a rhythmic embodiment of space and form," "in his color there is frankness and supernatural energy caught in a moment of eternal poise," "canvases which amalgamate and express with heroic precision the dreams of all their forerunners," "there is real fascination in the transitions for which he is famous," "a thorough master of his resources and uses them with consummate artistry," etc. The material for the artist Derain read in part as follows:

> It was in the ranks of the Fauves that he tasted the best and most complete of aesthetic creeds, disciplining his sensibilities by confronting them with the tendencies of eternal art. Gradually, however, the humanity of the artist proved its measured, its very French, flexibility and from then on . . . many were fascinated by his voluptuous robustness and natural health. He considers the plane of his canvas the essential soul of nature, the white surface as constituting a kind of ideal superspace, no longer of four but of an infinite number of dimensions. His sensibility refuses to be caged by any but the simplest formulas. It takes the form of an easy natural assurance, a spontaneous irresponsibility, and a complete absence of anything like presumption.

Type C, the mood and meaning of the picture, was in every case as restrained and valid as the experimenters could devise. Suggestions for the dominant mood were made by several competent observers, and the interpretation which seemed most likely to succeed was amplified and enlarged. For Monet, it

[2] In preparing the materials of Types A and B, the experimenters used besides several encyclopedias and current magazines the following books: Bulliet, C. J., *The significant moderns*, New York: Covici Friede, 1936; Fauré, Elie, *History of art*, tr. by Walter Pach, Vols. IV and V, New York: Harper, 1921; Raynal, Maurice, *Modern French painting*, tr. by Ralph Roeder, New York: Brentano, 1928; Wright, W. H., *Modern painting*, New York: Dodd Mead and Co., 1915; and others.

was the warmth, brightness and radiance of the summer air, and the richness and gayety which were emphasized. For Rockwell Kent it was the coldness, dignity, severity and loneliness, especially in relation to his blue color scheme.

The formal analysis of the picture, Type D, included a small sketch of the painting which showed the major elements of the composition and their relation to each other, the direction of the most important lines and as much as was feasible of the compositional scheme.[3] This sketch was followed by a description of the lines and masses, foreground and background, color scheme and color contrasts, balance, perspective, brushwork, patterns, volumes, etc. Everything was made very simple, straightforward and clear. Technical terms and clichés were avoided by repeated reference to the specific details of the picture. The objectives of the artist and the means by which he achieved them were set forth in concrete terms, in simple nontechnical language. At the same time, in view of the age and immaturity of the subjects, the sketches were not allowed to become merely cold laboratory dissections, but were given as much literary style as seemed appropriate.

THE TRAINING OF THE SUBJECTS

Since the factor of art training was important for our purposes, the subjects were asked to fill out an objective self-rating scale for their training in art. Most credit on this scale was given for formal training, studio work, college courses in art, architecture, etc. The scale included also credit for the reading of books and magazines on art, visits to museums and exhibits, memberships in camera and art clubs, and so on. The range of scores on this training scale was from 2 to 178, with the mean at 29.3 and a sigma of 25.8.

In tabulating the results of the experiment, the entire group of 180 subjects was divided into fourths on the basis of these training scores. Of the 45 students in the top quarter all had taken at least one course in the Fine Arts Department, and a large majority had had two or more courses. Only five students

[3] These sketches and all the other materials included in the formal analysis as well as many suggestions for the mood and meaning of the pictures were furnished by Professor Harry Engel of the Department of Fine Arts. Valuable criticisms and suggestions were also made by Leila Shelley Engel, and much of the success of the experiment is due to the work of these able critics.

in the second quarter and no student in the third or fourth quarter had had any course in art. The effects of formal training on the results of this study are, therefore, concentrated in the top quarter of the group.

The greatest difference in these groups lies between the first and second quarter. There is also a substantial drop in interest and training from the second to the third, but very little between the third and fourth. From the highest score of 21 to the lowest score of 2, there is very little differentiation in the students in these two lower quarters except in the more unimportant details of the magazines they read, the frequency of their trips to campus exhibits and their own self-rating of their interest and attitude. We have then in the top group the subjects who manifest a serious interest in art, possess a certain amount of manipulative skill and a familiarity with current and historical art forms. In the second quarter we find those with interest and pretensions, but little or no training. They are neither sophisticated as are those in the top quarter, nor naïve as those in the lowest groups. They take an occasional course, they do some reading in popular magazines on art subjects, they visit the exhibits and think of themselves as "much above average" in their interest. We might characterize them as the "dilettantes." Below them are the very naïve who do little reading, frankly rate themselves as average and make little or no pretensions as to their interest in art. The median for the top quarter is 57 ± 16, for the second quarter 27 ± 3, while for the combined third and fourth quarter it is 13 ± 2.

THE RATING OF THE PICTURES

On the first impression, with all the pictures presented anonymously, these 180 subjects showed the greatest preference for the pictures of Vlaminck and Kent. Monet and Cézanne were next in order of preference, reaching the point described as "pleasing but would not care to own." Kent is never rated below the neutral point, and only one or two subjects give Vlaminck a rating of neutral or below. On the other hand, Derain, frequently rated as disliked, in only one or two cases gets a rating as high as 4 or 5. His average falls a little above the neutral point. In general the order of preference for the five pictures follows the degree of photographic realism.

On the average, all the paintings gained in favor with the reading of the material. Kent, the most preferred, gained the most, and Derain the least popular, gained the next largest proportion.

THE EFFECT OF THE APPRECIATION AIDS

When the gains were tabulated according to the type of material which the subject studied, it was discovered that the gain in the case of the paintings for which no aid to appreciation had been provided, was on the average 0.11 of a step.

TABLE 1

GAINS EXPRESSED IN PROPORTION TO CONTROL GAINS CLASSIFIED BY TYPE OF APPRECIATION AID AND BY TRAINING GROUP AND BY SEX

	TYPE A PAINTER'S LIFE	TYPE B INTERPRETATION	TYPE C MOOD AND MEANING	TYPE D FORMAL ANALYSIS	AVERAGE GAIN
Top Quarter	1.8	3.4	2.6	6.8	3.7
Second Quarter	2.6	5.2	1.8	0.2	2.5
Third and Fourth Quarters	4.9	6.0	6.2	4.6	5.4
Average	3.6	5.2	4.2	4.0	4.3
60 Men [4]	3.8	6.2	2.6	5.3	4.5
60 Women	3.5	3.6	6.8	3.2	4.3

This gain showed practically no fluctuation from the top quarter to the bottom quarter of the group, and therefore the gains on each of the four types of material are expressed as proportions of this control gain of 0.11 steps. In Table 1 the gains for each fourth of the group are given for each type of material. For example, the total gain for the 45 persons in the top quarter who read Type A, was 9 steps, or on the average 0.2 of a step. Since this is 1.8 times the gain when no material was provided, it appears in the table as 1.8.

[4] Of the 180 subjects who participated in the experiment, exactly 60 were men and 120 women. The training scores for the women were noticeably higher than those for the men; their mean score was 32 and the mean for the men, 23.8. Therefore, in comparing the results of men and women, 60 papers were selected from the 120 women which matched score for score, the training scores of the men. In this way the effect of training was held constant for both groups.

It is apparent that the Interpretation Type of material is on the whole most useful in making the subjects express preferences for a picture. The average gain for this type of aid is 5.2, which is about 25 per cent better than the next highest type, Mood and Meaning with 4.2. The Life of the painter shows the least gain with 3.6, only 70 per cent as effective as the Interpretation Type.

For the top quarter, the most effective material is the Formal Analysis, Type D, and for the second quarter, the dilettantes, the Interpretation is by far the most effective. In the lowest quarters, the naïve group, no one type stands out as particularly better than any other. In other words, the Formal Analysis, Type D, is valued alike by those who have begun a serious study of art (top quarter) and by the very naïve, while the middle group, those who have some pretensions but little training, find these direct and specific materials not at all to their taste.

The greatest differences for men and women appears with Type C, the Mood and Meaning of the picture, for the women find this material almost seven times as effective as when no material is provided and the men not quite three times as effective. Men, however, are much more easily persuaded by Type B.

The average gains are somewhat larger for those with less training, as would be expected, because some of the material provided in these appreciation aids was already familiar to many of the students in the top quarter, and its effect, if any, on their judgment would show in their first rather than in their final impression. The dilettantes, the second quarter, gain the least, only about half as much as the more naïve subjects in the lower half of the group.

PREFERENCES OF THE SUBJECTS FOR THE DIFFERENT TYPES OF MATERIALS

In connection with the effectiveness of these different types it is important to note the preferences expressed for them in response to the question:

Place the schedule number of the material you found most interesting and helpful in the space numbered 1, the next most interesting in the space numbered 2, the least interesting in the space numbered 4, etc. 1..... 2.....3.....4.....

Table 2 gives the preferences for each successive group and for the men and women separately. In tabulating the preferences, the first place was weighted by four, second place by three, third place by two and fourth place by one. The figures in the table represent the preferences in terms of the maximum preference which any type of material could have received had every member of the group given it first place.

TABLE 2

Preferences for Types of Materials as Per Cent of Possible Maximum Classified by Training Group and by Sex

	Life	Interpretation	Mood	Form
Top Quarter	58	61	61	71
Second Quarter	60	65	63	61
Third and Fourth Quarters	65	53	66	64
Average	62	58	64	65
60 Men	58	62	57	74
60 Women	67	55	69	58

There are several significant facts to be drawn from this table. First of all we may notice that the group as a whole enjoyed *least* of all the Interpretation Type of material although the paintings increased in aesthetic value *more* from this type than from any other. Women and the more untrained subjects especially dislike to read this material. Those with much training prefer the Formal Analysis. Men also prefer the Formal Analysis and Interpretation, while women like best the Mood and Meaning and the Life. If we combine Types A and B, both of which are far removed from the specific picture, and Types C and D, both more pertinent, more direct and objective, we find that the more sophisticated and the more naïve care less for Types A and B (59 per cent and 59 per cent) while the dilettantes like them better (65 per cent). Likewise the dilettantes do not care for Types C and D (62 per cent) as much as either of the other two groups (66 per cent and 65 per cent).

A decided preference was shown for all material on both Kent and Vlaminck throughout all four groups. Their index

is 70 per cent, while those for Monet, Cézanne and Derain are 59, 56 and 55, respectively. The materials which they find most interesting are again not necessarily those which enhance the painter's work, for in the case of Vlaminck his materials were most preferred while his gains are only moderate (2.5) in respect to those of Kent (6.7) and to those of Derain (4.9) whose materials were the least interesting of any.

DISCUSSION AND CONCLUSIONS

It is not easy to make quantitative studies of the process of aesthetic judgment because it is a very complex process, and even for experimental purposes it should remain so. In this experiment the attempt was not so much to isolate one single variable and study it under systematically changing conditions, as to observe the effect, under controlled conditions, of some of the conventional materials of art appreciation and criticism.

The rating scale underlying the gains we present in our tables is a subjective one. We have assumed that the steps are quantitatively equal, an assumption which is open to the severest criticism. A similar and equally vulnerable assumption has been made in regard to the self-rating training scale. We have, therefore, given our data only the most simple and obvious tabulation and treatment, and it would be inadvisable to rely too heavily on these quantitative conclusions. Nevertheless, because our treatment is so simple, and our experimental situation so close to the actual classroom procedure in teaching art appreciation, our conclusions may have as much validity as the more objective but more remote observations of the laboratory.

It is noteworthy, first, that the preferences of observers is for realism. From the serious student to the naïve, through all grades of training and sophistication, photographic realism gains the greatest adherence. Even after the various excellences of the other three painters have been carefully explained, Kent and Vlaminck with their realism and their sunset and moonlight effects are given decided preference.

Since there are, of course, many ways of enjoying pictures, it would be arbitrary for the experimenters to pronounce any one of the four types of appreciation aids as most valid or least valid. It is fairly obvious, however, that Types A and B (1) are most remote from the actual aesthetic object, (2) are full of

empty phrases and meaningless clichés, and (3) originate from the professional critics and journalists rather than from practicing artists. Types C and D, on the other hand, (1) are directed toward each picture specifically, giving information and analysis of the excellences of each painting in clear and comprehensible terms, (2) employ only the concepts which the amateur can understand, and (3) explain the pictorial elements as most practicing artists would explain them.

The largest gains, for these pictures and these observers, come in response to the type of material which is (1) most remote from the actual aesthetic object, (2) least clear, appropriate and comprehensible to the subjects, and (3) least valid from the practicing artist's point of view. Apparently the layman expects art criticism and appreciation to be couched in certain characteristic terms; and although they may be largely incomprehensible to him and although he admits that he prefers to read other types of material, they do create a certain kind of effect which he counts as aesthetic. Platitudes, technical obscurities and philosophical mysteries, fine writing and glamour, represent the most effective medium for enhancing the aesthetic experience or at least for improving the observer's rating of what he thinks is aesthetically important. The prestige effect of these literary paragraphs is amazing.

In both cases—in the demand for photographic realism, as well as in the susceptibility to meaningless phrases in lieu of honest analysis and criticism—there is a disconcerting gap between artist and layman. The form and the expressiveness, which to the creator are the very essence of successful effect, the objectives and excellences which he most ardently pursues, these to the layman are of least significance, entirely subordinate to realistic effects. Similarly, the type of criticism and interpretation which is most effective for the layman, seems to the professional artist to be at the very least uncommendable, largely irrelevant and in many cases entirely meaningless.

In view of this gap between what the artist attempts and what the public wants, it is important to note that (1) the largest gain for any type or group is the Formal Analysis in the hands of the most sophisticated group, that (2) the most naïve profit more from Types C and D than do the dilettantes

whose sensibilities have perhaps been dulled by some of the ecstatic patter of the current (and classic) writers on art, and that (3) in general the observers do not prefer to read this type of material; in fact, they give it the lowest possible rating.

The psychologist recognizes the fact that the feeling of aspiration, an increment of novelty, and a touch of the mysterious and incomprehensible are important ingredients of the aesthetic experience. With complete mastery and understanding of the aesthetic object, the experience tends to fade. But there is a recognizable difference between an intelligent understanding which falls short of complete comprehension, and a merely glamourous effect associated only with the personality of the artist and the prestige of the critic. Here lies the difference between a stable and integrated function of the individual which allows further development of aesthetic experiences, and a capricious accumulation of phrase and anecdote divorced from the fundamental sensory and perceptual experience.[5] There is a gap everywhere between the artist and the layman which the teacher and interpreter must reckon with, and there is a group of dilettantes whose resistance will be the most difficult to eliminate. The authors are convinced that much of the current writings on art are probably increasing rather than diminishing this difficulty.

[5] See Hevner, Kate. The aesthetic experience, a psychological description. *Psychol. Rev.*, 1937, **44**, 245–263.

A CRITIQUE OF THE HUMM-WADSWORTH TEMPERAMENT SCALE

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IN a recent article Humm and Wadsworth published an account of the development and standardization of a Temperament Scale.[1] The scale was devised in connection with a personnel program in a public utility corporation and was designed to serve as a diagnostic aid in determining the temperamental fitness of employees for various types of work. The theoretical basis around which the scale was constructed was the theory of personality formulated by Rosanoff.[2] According to this theory each component of temperament is present in the personality organization of an individual. The relative degree of strength of each component may be considered a determining factor in behavior. Abnormal behavior is considered to be the result of the presence of one or more components to an excessive degree.

The normal individual, then, is the one within the organization of whose personality these components are reasonably well balanced. The diagnosis of temperament involves the identification of the relative strength of each component, regardless of whether or not the influence of the component is manifested in the behavior of the individual in question. These components are as follows: (1) "normal," a control mechanism involved in the maintenance of temperamental equilibrium; (2) hysteroid; (3) cycloid, which is sub-divided into (a) manic and (b) depressed; (4) schizoid, which is sub-divided into (a) autistic and (b) paranoid; and (5) epileptoid.

After considerable preliminary work these authors developed a scale consisting of 318 questions, each of which may be answered either "yes" or "no," of which 159 are scored. The scoring of each question is dependent upon the component or components

[1] Humm, D. G. and Wadsworth, G. W., Jr. The Humm-Wadsworth Temperament Scale. *Amer. J. Psychiat.*, 1935, **92**, 163–200.

[2] Rosanoff, A. J. *Manual of psychiatry*, 6th ed., Part II, Chap. 25, "A theory of personality based mainly on psychiatric experience." New York: Wiley, 1927.

touched upon and a statistical weighting depending on the significance of the question content. A profile is provided for use in expressing the relative strength of the components, the units of which range from −3 (indicating a marked weakness of the given factor) to +3 (indicating excessive strength of the factor). The norms given by the authors were based on results obtained from 436 individuals, including business employees (representing a "normal" group) and inmates of hospitals for the insane and penal institutions. The validity of the questions was studied by comparing the obtained scores with clinical data available for each subject.

In the study here reported the Humm-Wadsworth Temperament Scale was used with a group of university students. It was found that the results obtained from this group were not comparable to the published norms. There were several possible explanations for these observed differences. In the first place, the age factor was of possible significance, Humm and Wadsworth having stated that their norms were not suitable for use with those under 20 years of age. The second possibility was that the university group represented a selection of the population not comparable to that used in the original study. In addition, our experimental group was a mixed one. Since Humm and Wadsworth made no mention of the sex of the subjects in the standardization group, it is assumed that they were probably all males. Due to the observed differences the study was extended to include a sufficiently large number of subjects to permit a statistical treatment of the data. The validity of the classification of the questions into the seven components included in Rosanoff's theory of personality was provisionally accepted.[3]

The subjects used in this study may be divided into three groups. The first was composed of 148 undergraduate men, hereafter called the M group. The second included 106 undergraduate women, hereafter called the F group. The third group was made up of 53 women registered in a library school (the FL group), all of whom had completed undergraduate work. The mean ages of these groups were 20.4 years, 20.2 years, and

[3] At the present time Mr. R. W. Bornemeier and the author are factor-analyzing our data to determine whether or not these categories actually emerge from a statistical treatment of the results.

24.3 years, respectively. These age means are based on data furnished by about 90 per cent of the subjects in each group.

In an effort to avoid the inclusion of those results quantitatively invalid because of biased responses, the original authors established standards for the acceptance or rejection of a test based on the number of "no" responses and the algebraic sum of the deviations of the components in terms of profile score, excluding the "normal" component. A similar selection was originally attempted in this study, but due to the large percentage of "rejected" tests and the similarity in means between the "accepted" and "rejected" groups, the results obtained from all our subjects were accepted for consideration. It is distinctly possible that the 40 per cent of our subjects whose results fell within the "rejected" classification represented not a biased or uncooperative group, but rather were those whose scores were toward the extremes of the distribution. As will be indicated later, the reliability of these "rejected" scores was sufficiently high to justify their inclusion.

The results obtained from our groups are given in Table 1. The data are tabulated according to the strength of each component and rated from +3 to —3 accordingly.

It is obvious from an examination of this table that the scores made by those composing our groups are, in most instances, unduly concentrated at one extreme or the other. This tendency is most apparent in respect to the cycloid manic, cycloid depressed, and epileptoid components, an unusually large number of scores appearing at the upper limits in the first two of these, and toward the lower values in the third. The scores on the cycloid depressed component are materially higher, particularly in the F and M groups. In both the F and FL groups the scores on the H component are concentrated at the lower end of the scale, while those for the M group are more nearly comparable to the norms of the original group. It is obvious, however, that the original norms do not furnish an adequate basis of comparison for the scores made by the student groups. Otherwise one must draw the conclusion that the university groups were composed largely of individuals with marked cycloid temperaments, often approaching the pathological in degree. It is possible that a university or college atmosphere tends toward the exaggeration

of such tendencies. It is more probable, however, to maintain that we are here dealing with a selected group for whom separate norms are required.

TABLE 1

TABULATION OF SCORES ACCORDING TO THE STRENGTH OF EACH COMPONENT

	N	*H*	*M*	*D*	*A*	*P*	*E*
F GROUP							
+3	7	0	32	22	1	6	0
+2	33	6	33	22	12	14	2
+1	15	5	23	24	26	9	3
0	28	35	12	14	12	27	23
−1	15	35	3	12	28	9	35
−2	7	17	2	9	11	29	33
−3	1	8	1	3	16	12	10
M GROUP							
+3	10	4	58	27	2	27	0
+2	18	36	43	32	17	34	0
+1	12	24	33	37	33	10	10
0	50	56	6	25	24	36	37
−1	30	19	4	16	27	5	36
−2	18	7	4	6	25	31	45
−3	10	2	0	5	20	5	20
FL GROUP							
+3	8	0	15	6	0	1	0
+2	23	0	14	9	3	2	0
+1	6	3	12	11	13	3	2
0	12	7	6	13	11	9	15
−1	3	14	3	7	9	1	8
−2	1	19	3	6	11	22	19
−3	0	10	0	1	6	15	9

A statistical statement of the distribution of scores for each of the components is given in Table 2.

Since Humm and Wadsworth do not give statistical data on the distribution of scores for the various components obtained from their group of subjects, it is impossible to make a statistical comparison of the groups. The significance of the differences between the means of the various groups in this study is given

TABLE 2

DISTRIBUTION OF SCORES FOR EACH COMPONENT

	M GROUP		F GROUP		FL GROUP	
	MEAN	S.D.	MEAN	S.D.	MEAN	S.D.
Normal	37.24	9.86	41.62	8.71	46.55	7.65
Hysteroid	48.21	11.83	36.77	10.91	29.02	10.73
Cycloid manic	37.78	7.98	36.45	8.73	35.30	9.39
Cycloid depressed	49.16	16.56	48.90	17.36	45.06	15.94
Schizoid autistic	37.88	15.23	37.86	15.25	37.80	14.81
Schizoid paranoid	28.55	8.41	23.58	9.05	18.11	8.16
Epileptoid	24.36	7.60	24.99	6.74	23.55	7.55

in Table 3. Comparisons have been made between the means of the two undergraduate groups (F and M) and the two groups of women (F and FL).

It is apparent from an examination of Tables 2 and 3 that significant differences exist between these groups in regard to several of the components. The F group shows a significantly higher mean for the "normal" component than does the M group, and the FL group mean is in turn significantly higher than that for the F group. This finding would seem to indicate a sex difference in respect to the characteristics measured by this component.

The most marked differences between these groups appear in the hysteroid component. There is a statistically significant difference between the mean scores of the two undergraduate

TABLE 3

SIGNIFICANCE OF DIFFERENCES BETWEEN MEANS FOR EACH COMPONENT

	F AND M GROUPS			F AND FL GROUPS		
	S.D. diff.	Actual diff.	Critical ratio	S.D. diff.	Actual diff.	Critical ratio
N.	1.176	4.38	3.72	1.348	4.93	3.66
H.	1.438	11.44	7.96	1.812	7.75	4.28
M.	1.072	1.42	1.32	1.544	1.15	0.75
D.	2.164	0.26	0.12	2.762	3.84	1.39
A.	1.939	0.02	0.01	2.531	0.06	0.02
P.	1.118	4.97	4.45	1.424	5.47	3.84
E.	0.905	0.63	0.70	1.229	1.44	1.17

groups, although both are within the "borderline" classification as given by Humm and Wadsworth. The mean for the FL group is significantly lower than that of the F group, the former being in the −2 classification according to the original standardization. There appears to be no ready explanation for these differences, particularly that between the undergraduate groups. It is possible that the anti-social tendencies which are supposedly tapped by the questions scored for this component are more pronounced among university men than university women. If the scale was designed for use with men only and standardized accordingly, then these differences may merely reflect differences in the groups considered. It is doubtful that hysteroid tendencies are so much more pronounced in men than among women. If the scale was standardized on a male group, then the observed differences may be explained as being due to the fact that the questions more thoroughly explore the types of hysteroid tendencies prevalent among men than among women. It is hoped that the statistical analysis now in progress will shed additional light on this question.

The mean scores on the cycloid manic component for all three groups fall in the +2 classification according to the original standardization. The means for the cycloid depressed component fall in the +1 classification. There are no statistically significant differences between the means of our groups. This circumstance would seem to indicate that university students show unusual strength in both the manic and depressed phases of the cycloid component. Humm and Wadsworth have characterized the manic phase as indicative of ". . . some degree of elation, pressure of activity and distractibility, together with such manifestations of excitement as jests, pranks, enthusiasms, impatience, etc." The depressed phase is similarly characterized by ". . . some degree of sadness, lessened activity, dearth of ideas and associated characteristics such as worry, timidity, feelings of malaise, and the like." The cycloid temperament in general is marked by ". . . fluctuations from emotional equilibrium, hotheadedness, difficulty in sleeping, etc."

Whether such characteristics of temperament are more prevalent among university groups than among industrial workers is a question. It is possible that the emotional drive and versatility

which Humm and Wadsworth maintain is measured in this component is a factor which operates in the selection of students as a group. Another factor which must be considered is the age of the student groups. As mentioned before, the mean age of both the F and the M groups is slightly above 20, below which point the original authors maintain the norms cannot be used. In this connection it is to be noted that none of the students in our three groups were freshmen. In order to determine whether these high scores on the cycloid component are of particular significance it would be necessary to compare these scores with those obtained from a non-university group of the same age.

Significant differences are also found between our groups in respect to the schizoid paranoid component. Although the means for both the F and M groups are within the "borderline" classification, the former is significantly lower than the latter. The mean for the FL group is significantly lower than that for the F group, and is of −2 strength. This fact would seem to indicate that university men are more suspicious, conceited, and intolerant of the opinions of others than are university women. An analysis of the items scored for this component may supply an explanation of these differences.

There are no significant differences between the means of our groups in either the schizoid autistic or the epileptoid components. These means are somewhat lower than the midpoints of the distributions of scores on the original standardization. In the epileptoid component our distributions show a conspicuous lack of scores in the +2 and +3 categories. Here is one more indication that the original norms are not suitable as a basis of comparison for scores made by university students.

The reliability of the scores for each component was determined through the use of retests. The coefficients of correlation between the scores made on the two occasions are given in Table 4.

TABLE 4

RETEST RELIABILITY OF EACH COMPONENT

Normal	.858±.031
Hysteroid	.850±.032
Cycloid manic	.919±.018
Cycloid depressed	.866±.029
Schizoid autistic	.878±.027
Schizoid paranoid	.883±.025
Epileptoid	.666±.064

Although the number of subjects included in this group was only 34, all seven of the coefficients are statistically significant. The interval between test and retest was three months or more in every case. These coefficients of reliability compare very favorably with those found by Humm and Wadsworth using the split-half method and are quite high for a scale of this type. The reliability of the cycloid manic component is somewhat higher and that for the epileptoid component is considerably lower. It is apparent that all seven of the components of the scale show a high reliability. In only 10 of the 34 cases here

TABLE 5

INTERCORRELATIONS BETWEEN COMPONENTS

	H.	M.	D.	A.	P.	E.
N.	—.596±.029	—.587±.029	—.647±.026	—.342±.039	—.785±.017	—.218±.042
H.		+.301±.040	+.265±.041	+.001±.044	+.656±.025	+.089±.044
M.			+.853±.007	+.380±.038	+.436±.036	+.092±.044
D.				+.619±.027	+.444±.036	+.204±.042
A.					+.174±.043	+.037±.044
P.						+.094±.044

considered could both the test and retest scores be classified as "acceptable" according to the standards of the authors, based on the number of "no" responses and the profile scores. It would be reasonable to expect low reliability if the tests were found to be quantitatively unacceptable. In a test composed of 318 questions it is difficult to believe that the memory factor would be of particular importance except in respect to some few items of unusual significance to the subject. On the contrary it would seem reasonable to expect that carelessness in replying to the questions would result in low reliability. The high reliability of these "unacceptable" scores was one of the chief reasons for disregarding these standards and including all scores in our summaries.

The intercorrelations of the scores on the various components are given in Table 5. These data are based on the scores of 232 of the subjects of the three groups.

There are some marked differences between these coefficients and those given by Humm and Wadsworth. This is particularly

true in respect to the correlations between the "normal" component and the remaining six. Humm and Wadsworth found these to vary from +.044 (with the epileptoid score) to +.694 (with the cycloid manic score). Briefly, they found moderately high positive correlations, while those obtained from our data are all negative, and in most instances somewhat higher. If, as the original authors maintain, the "normal" factor ". . . is primarily a control mechanism, providing rational balance and temperamental equilibrium," then the negative relationship appears to be more readily understandable than the positive coefficients. Solely on a logical basis it seems hardly reasonable to expect a high score on the cycloid manic component to be accompanied by a high score on a component in which such a score is indicative of a high degree of self-control and temperamental equilibrium. Marked cycloid tendencies on the part of an individual without adequate self-control would seem to warrant the expectation of behavior at least bordering on the abnormal. It is possible that the age of those included in our groups was such that the type of self-control and equilibrium measured by this component had not reached the level of development found in the older standardization group.

The remaining intercorrelations are, in most cases, quite comparable. Differences which are, in all probability, statistically significant appear in several instances, but previously mentioned differences between the groups of subjects used may account for them.

One intercorrelation which seems to warrant particular mention is that found between the scores on the cycloid manic and the cycloid depressed components. Humm and Wadsworth found this to be +.887, while our data reveal a coefficient of +.853. Coefficients of correlation of this magnitude between personality or temperamental characteristics are quite unusual, and one may question the validity of dealing with these components separately. On the basis of the figures it would appear to be more reasonable to deal with the cycloid component as a single one. It is possible that the analysis of our data will throw additional light on this point.

In an effort to account for some of the observed differences between the standardization group and the students who composed our three groups, the scores on the various components

were correlated with the scores on the Army Alpha Intelligence Test (Nebraska Revision). Although it seems to be generally agreed that a university group represents a selected sampling of the population in respect to intelligence, yet the range of scores within our groups (106 to 200) was sufficiently great to be indicative of any actual relationship. Scores were available for 148 of the subjects of the F and M groups, and the obtained coefficients of correlation are given in Table 6.

TABLE 6

CORRELATIONS BETWEEN ARMY ALPHA SCORES AND THOSE MADE ON THE VARIOUS COMPONENTS

Normal	$+.225 \pm .053$
Hysteroid	$-.062 \pm .055$
Cycloid manic	$-.115 \pm .055$
Cycloid depressed	$-.121 \pm .055$
Schizoid autistic	$-.081 \pm .055$
Schizoid paranoid	$-.117 \pm .055$
Epileptoid	$-.182 \pm .054$

The only coefficient which is statistically significant is that between the Army Alpha score and the score on the "normal" component, which is 4.2 times its probable error. This coefficient is so low, however, that the indicated relationship is very slight. The remaining coefficients are negative, but not statistically significant. It seems apparent from these data that the scores made on all components are independent of the intelligence rating of the individual. Some other basis for an explanation of the differences between our groups and the standardization group must be sought. Even in the development of those characteristics measured by the "normal" component, i.e. self control and temperamental equilibrium, the factor of intelligence is of only slight importance.

SUMMARY

It is evident that our data gathered from a total of 307 university students differ in many respects from those obtained in the original standardization of the scale. A detailed statistical comparison cannot be made because Humm and Wadsworth have not given the means and standard deviations of their distributions of scores. Thus one is forced to rely on a comparison of the distributions when the results are classified into the seven categories of strength in accordance with the method used in the standardization of the scale. When this is done, our distri-

butions show an undue concentration of scores at one extreme or the other in several of the components. Whether this effect is due to the age of our subjects, the inclusion of both sexes in our groups, the selection of subjects, or the operation of all of these factors is not immediately apparent. It would seem, however, that if the scale is to be used as a diagnostic aid in evaluating the temperamental characteristics of industrial workers, there is need of standardization data based on results obtained from a broader sampling of the general population.

The method used by Humm and Wadsworth in determining the categories of strength of component is, in itself, questionable. Although the method is not described in detail, it seems that the authors divided the scores on each component into two groups, one made up of those scores above the median or average and the other of those scores below the median or average. Each of these was then considered individually, and the limits of each category were based on the standard deviation of each of these groups. This procedure is questionable because it actually amounts to a consideration of each half of the total distribution as a unit conforming more or less closely to a normal distribution. Unless the distribution of scores for the entire component is bi-modal, such procedure is statistically questionable.

There are several characteristics of the scale which are such as to warrant further investigation as to its usefulness. The reliability of the scores on the various components is high, being .850 or above for all components except epileptoid. These coefficients of reliability compare very favorably with those obtained by the original authors. A second consideration is the lack of any significant correlation between the scores on the various components and intelligence test scores. The correlation between the latter and the scores on the "normal" component is statistically significant, but since the coefficient was found to be only +.225, the relative independence of the two is apparent. The remaining coefficients were negative and not statistically significant. These factors, together with the low coefficients of correlation between components, seem to warrant the conclusion that the scale is actually probing various phases of personality. It is hoped that the statistical analysis now in progress will determine the validity of scoring the scale for the seven components accepted by the original authors.

THE EFFECT ON THE BERGER RHYTHM OF MILD AFFECTIVE STATES*

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THE purpose of this study was to discover the possible relationships between brain potential patterning and mild affective states. In many types of psychological experimentation the importance of the affective component must be considered—a fact which is especially true in working with human beings.

Inherent in the situation in which brain potentials may be taken are factors that might give rise to such states as fear, competition, and excitement. It was first pointed out by Berger that the regularity and amplitude of the alpha rhythm in man can usually be observed best under optimum conditions of relaxation (4). Apprehension, worry, and excitement tend to depress or to disrupt the rhythm. The basis for this disruption, according to Jasper (7), does not seem to be related to the usually-recognized signs of relaxation or excitement.

Berger reports that fright will cause the disappearance of the alpha waves (5). Following the explosion of a fire cracker near the subject, the waves disappeared for a period of several seconds. Thereafter, for some time, there were alternating periods of appearance and disappearance of the rhythm. In the present study, however, no attempt was made to induce such intense emotional states. Rather it was the purpose to investigate the effect on the brain potentials of mild changes in affect.

Forty-four subjects, mostly college upperclassmen and graduate students, were used in the experiments reported here. Nineteen were women and 25 men. They were directed to recline quietly on a cot, with eyes closed, in an electro-statically shielded, semi-soundproofed room. The brain potentials were recorded with one electrode on the left occiput and the other on the lobe of one ear.

* Grateful acknowledgment is made to Professor Lee Edward Travis for his assistance in this study.

RECOGNITION OF LIES BY BRAIN POTENTIAL RECORD

To investigate the possible effects of a situation involving a simple lie on brain potentials, records were obtained from 33 adult subjects in 90 lying situations. A typical procedure consisted of showing the subject one of five cards on which numbers had been printed. The cards were arranged in such a way that the experimenter did not know which card was being seen by the subject. Through a two-way communicating system the numbers on the cards were then read by the experimenter.

TABLE 1

SUMMARY OF RANKINGS OF THE LIE RECORDS

RANKING OF THE LIE RECORD	CHANCE DISTRIBUTION	JUDGES					
		I	II	III	IV	V	Means
First	18	43	35	45	42	42	41.4
Second	18	15	25	15	19	15	17.8
Third	18	20	16	13	9	18	15.2
Fourth	18	8	8	9	10	6	8.2
Fifth	18	4	6	8	10	9	7.4
N	90	90	90	90	90	90	90
Average ranking	3	2.06	2.17	2.11	2.19	2.17	2.11
Chi squared value	0	51.9	38.1	52.4	44.3	45.0	43.2

After each number was read, the subject responded with "no," indicating that he had not seen the number just read. On one number a lie was inevitable. A brain potential record of a duration of about 20 seconds was taken after each response.

The possibility that the startle effect incident to the beginning of the questioning might introduce a variable factor in evaluating the records, was obviated by reading first a number which had not been included on the cards and which the subject, therefore, had had no chance to see. This first record was not included among those which later were to be compared for the purpose of trying to recognize the one taken during the lie. Two or three situations were set up for each subject.

Five people, experienced in evaluating brain potential rhythms,

independently compared the records for the purpose of locating those in which the lies were involved. Using the general depression or disturbance of the alpha rhythm as a criterion, the records from each situation were ranked from one to five. The record considered most likely to be the one taken when the lie was being told was ranked first, and the one judged least likely was ranked fifth. The remaining three were ranked in order between these two. Table 1 shows the results of these judgments.

Each of the judges ranked the lie record in first place more often than in any other place, the average number of times it was so ranked being 41.4. This was 47 per cent of the time, or 130 per cent more often than could have been expected by chance. Had a chance distribution of ranks been made, the average ranking of the lie record would have been 3.00, but the actual performance of the judges showed an average of 2.11.

Another way of evaluating these data was to obtain the values of the chi squared measure of the goodness of fit. The assumption was made that the chance distribution of the lie records among the various ranks would have been as shown. The greater the chi squared values, the greater are the differences between the chance distribution of rankings and the empirical results of the judges' rankings of the lie records. The values shown for chi squared indicate that such deviations from a chance distribution would occur less than once in 100,000 times (8).

The judges varied in ability to recognize the lie record. In general, the more experienced the individual in working with brain potential records, the better was his performance.

When the average ranking of the lie record by the five judges was computed for each subject, it was found that the records of some individuals were consistently more easily recognized than were others'. Four of the subjects had records such that all judges could recognize the lie record with almost complete certainty, while on the records of five of the subjects the judges gave a worse than chance performance.

For the purpose of observing whether something similar to adaptation or readjustment in the brain potentials—as reported by Bagchi (2)—would take place during a series of lying situations, nine such situations were set up for four subjects. The

records were ranked and these rankings grouped so that the results on the first three situations could be compared with the results on the last three. The performance of the judges in recognizing the lie record was 40 per cent better on the first three situations than on the last three.

DETECTION OF GUILT BY BRAIN POTENTIAL RECORDS

In order to investigate whether guilt could be detected from brain potential records, 19 subjects were projected into experimental misdemeanors, and records of their brain potentials taken while key words from these situations were read to them. Five misdemeanors were described on separate cards. One of the descriptions used was as follows:

> You have been living at a fraternity house. One Saturday evening you have to hurry down town to buy some notebook paper before the stores close. You know that you cannot get there in time if you walk. One of the members has a Buick car standing at the curb. You decide to drive it down without asking permission. Coming home, you round an icy corner, suddenly lose control, and smash into a lamp post, crushing a rear fender. Nobody is near, so you decide to drive the car on home, park it, and pretend to know nothing about the damage. The next morning you are questioned about the damaged fender. You are suspected of having taken the car.

The subject, after being permitted to read one of these descriptions, was asked to assume that he had committed the act described. Twenty-five words and phrases, five from each situation, were arranged in random order. The five phrases drawn from the description quoted above were "Saturday evening," "Buick car," "Crushed fender," "Lamp post," "Icy corner." While the subject merely listened, this list was read to him. After each word or phrase, about 15 sec. of brain potential record was taken. These records were then so arranged that they were in groups of five, each group corresponding to a misdemeanor.

The same judges independently compared the records and ranked the five groups from each subject from one to five, placing first in rank the group that appeared to be the one related to the misdemeanor the subject was supposed to have committed. The general depression or disturbance of the alpha rhythm was used as the criterion. Table 2 shows the distribution of the rankings made by these judges.

The guilt record was recognized, on the average, 48 per cent of the time, or 140 per cent more often than could be expected by chance. One judge was able to recognize the correct record 68 per cent of the time. The average ranking of the guilt record by the judges was 2.16. Chance would have given an average ranking of 3.00.

By chance, the chi squared values indicate that a distribution such as was given by the judge who was the least able to recognize the guilt records would occur once in ten trials; by the most competent judge, four times in 10,000 trials; and by the average of all the judges, three times in 100 trials.

TABLE 2

Summary of the Rankings of the Guilt Records

Ranking of the guilt record	Chance distribution	Judges					
		I	II	III	IV	V	Means
First	4.75	13	5	9	8	10	9.0
Second	4.75	1	8	3	4	3	3.8
Third	4.75	2	1	2	2	2	2.0
Fourth	4.75	2	0	5	2	3	2.4
Fifth	4.75	1	5	0	3	0	1.8
N	19	19	19	19	19	19	19
Average ranking	3.00	1.79	2.57	2.15	2.37	1.90	2.16
Chi squared value	0	23.3	10.0	10.7	6.2	12.5	8.6

The ranks given by the judges for each guilt record were averaged to show that the records for some subjects were much more reliably read than were those for others. The records of six of the subjects were such that all judges recognized the guilt records with almost complete accuracy. One subject's record was consistently ranked too low by all of the judges.

Brain Potentials During Free-Association

In order to investigate the characteristics of the brain waves during a free-association test, a 50-word list was administered to 12 subjects. The list was so constructed that about an equal

number of words of pleasant and unpleasant affect was included. Typical words were "beauty" and "blood."

The subject was instructed to respond promptly with the first word that occurred to him after each stimulus word was given. A signal key, operated by the experimenter, indicated on the record when the stimulus words and the responses were given, the record being run after each word until the return of the alpha rhythm. The records were subsequently analyzed to determine the relationships existing among the latency after the stimulus word, the perseveration time, and the reaction time. The latency is considered to be the temporal interval between the instant of the appearance of the stimulus and the obliteration or striking depression of the brain potentials. Perseveration time is the temporal interval between the cessation of the stimulus and the reappearance of the alpha rhythm. The reaction time is the temporal interval between the instant of the appearance of the stimulus and the verbal response.

The coefficient of correlation between perseveration and reaction time was $.27 \pm .027$; between perseveration and latency, $-.09 \pm .032$; between reaction time and latency, $.13 \pm .032$.

Most of the words resulted in some depression of the alpha rhythm. There were, however, wide individual differences among the subjects in this respect. For one subject only 30 per cent of the words were effective in depressing the rhythm.

Nine of the subjects were directed to report an introspective rating of the emotional affect of each stimulus word in the list used, the report being requested after the return of the alpha rhythm following the response word. These reports were taken in order that the relationships that might exist between the ratings and the effects on the alpha rhythm of the stimulus words could be studied. The rating was made along a pleasant-unpleasant continuum of seven steps, extending from a plus three through zero to a minus three. Minus three indicated a word of extremely unpleasant affect, while plus three indicated a word of extremely pleasant affect. When zero was reported, the word carried a neutral affect for the subject.

From the subjects' ratings it seemed that about an equal number of words of pleasant and unpleasant affect was included in the list. On 46 per cent of the words no unpleasant affective

ratings were reported, and on 36 per cent no pleasant affective ratings were given by any of the nine subjects. The coefficients of correlation between these ratings and the reaction time, and between the ratings and perseveration time were very nearly zero.

No consistent effects were observed on the alpha rhythm after many of the stimulus words. This lack of effect was evident at all levels of the ratings given by the subjects. Table 3 shows the percentage of ineffective stimulus words at the various ratings for all the subjects.

TABLE 3

Percentage of Ineffective Words at Various Ratings

RATINGS	TOTAL WORDS RATED	PERCENTAGE OF WORDS INEFFECTIVE
+3	82	14.5
+2	83	16.9
+1	44	11.4
0	52	19.2
−1	49	6.1
−2	64	10.9
−3	68	22.1
	Total: 442	Avg.: 14.9

Brain Potential Variations with Sentence Type

For the purpose of studying the effect of different types of reading material on the brain potential patterning, records were taken from 10 subjects while they listened to the reading of 30 sentences. These sentences were divided into the following groups: 10 impersonal sentences, consisting of five questions and five statements; 10 impersonal sentences, of which five were true and five false; and 10 questions, five personal and five impersonal. The sentences in each group were arranged in random order. The following sentences are typical of those used:

Impersonal question: "Do two and three make five?"
Impersonal statement: "This is not such a bad day."
True statement: "Sea water has a salty taste."

False statement: "Apples are vegetables."
Impersonal question: "Do growing plants need sunshine?"
Personal question: "Are you considered to be good-looking?"

The percentage of the time the alpha rhythm was absent or depressed was calculated for the first five seconds, the second five seconds, and the whole 10 seconds after each sentence was completed. The means of these percentages were found for each of the types of sentences. The result showed, however, that the differences among the means of the percentages for each of the types of sentences were not significant for any period of time calculated.

TABLE 4

CRITICAL RATIOS FOR THE DIFFERENCES IN PERCENTAGE OF EFFECTIVENESS IN DEPRESSING THE ALPHA RHYTHM

TYPES OF SENTENCES	DIFFERENCES IN PERCENTAGES	C.R.
Personal questions vs. questions	26	2.9
Personal questions vs. statements	36	4.0
Personal questions vs. true statements	24	2.7
Personal questions vs. false statements	28	3.1
Personal questions vs. impersonal questions	22	2.5

The records were inspected for the purpose of observing the frequency with which the alpha rhythm was depressed before the completion of the sentence. These percentages ranged from 46 per cent for the unemotional statements to 82 per cent for the personal questions. The personal questions were significantly more effective in depressing the rhythm before completion of the sentences than were any of the other types of sentences. The critical ratios for the differences in percentage of effectiveness are shown in Table 4.

DISCUSSION

One of the striking facts revealed in the analysis of the 925 records taken in connection with the lie and guilt detection experiments was that the quality of the rhythm produced by the subject was not important. About half of the subjects whose

lie and guilt records were most successfully recognized by the judges belonged to the "rare alpha" group. Conversely, over half of the subjects whose lie and guilt records were not successfully recognized had "good" alpha rhythms.

It has been assumed, particularly by Adrian (1), that the alpha rhythm represents the synchronous discharges of the brain cells, and that the smaller, faster and more irregular rhythm represents a breakup of the collective action. From this supposition it would seem logical to expect that the records of the subjects producing a "good" alpha rhythm should have been more easily disturbed and consequently more accurately judged than those from subjects producing "poor" alpha waves, because the latter group's waves were already largely broken up. Since this was not the case, it may be that the "poor" alpha rhythms are as stable a form of electrical discharge and as subject to alteration as are "good" alpha waves. "Good" alpha patterns might, however, indicate a high degree of stability that is difficult to disturb, while "poor" alpha rhythms might indicate a high degree of instability.

The three subjects whose brain potential patterns were disturbed by the least number of words in the free-association test, were those whose records showed the most regular alpha rhythm. This fact may be considered as further evidence that pronounced alpha rhythms are no more easily disturbed than less regular rhythms.

Another important factor that probably influenced the certainty with which the lie or guilt record could be recognized was the subjective significance to the subject of telling the lie or assuming the guilty role. One subject, an ex-minister, reported a very marked aversion to telling the lies. His lie records were recognized with perfect accuracy by all the judges. Several subjects reported that they attached no importance to lying under the conditions set up. It is likely that the lie or guilt record could be much more reliably recognized if the situation were more crucial. The one subject's records that were ranked consistently too low apparently required a different criterion from that which was being used by the judges to detect the guilt record. The usual criterion used was the general depression of the waves. It was quite often observed, however,

that only that part of the record taken the first second or two after the lie was of any differentiating value. There were wide individual differences in this respect. The rhythm of some subjects continued to be altered for several seconds after the lie.

The judges could usually point out distinctive characteristics of the brain waves to justify their rankings of the lie or guilt records. These characteristics included unusually abrupt or prolonged depression or alteration of the rhythm. It is probable that better methods of evaluating the records would yield more reliable results, for those of the judges who exercised greatest care and were most experienced in evaluating brain potential patterns produced significantly better results than did the others.

Two other methods of measuring the brain potential patterns were used; the total excursions of the waves on each record were determined by measurement, and the percentage of the time the alpha rhythm was altered was calculated. Neither method proved as good as that involving the subjective judgments of the judges.

The variations in the brain potential patterns of the subjects were very marked in the free-association test. For some of the subjects, many of the words apparently did not disturb the alpha rhythm at all. These same subjects, however, gave a full range of affective ratings on these words. It is apparent that there is no consistent relationship between the subjective ratings given and the measures taken on the brain potential patterns.

Hull and Lugoff state that the length of time it takes a subject to respond is the best indication of the emotional affect of the word in a free-association test (6). Yet in the brain potential measures, neither perseveration time nor latency correlates significantly with this criterion.

In constructing the sentences used in the last section of this report all except the personal questions were designed to be free from emotional affect. It can never be certainly determined, however, that this effect has been accomplished. It is probable that further work with various types of sentences would yield more positive results if questions or statements were used more deeply personal in nature than those included in this study. Perhaps other measures of the effects of sentences on the brain potentials should be obtained. The comparisons made between

the records for the various types of sentences, on the basis of the frequency with which they depressed the alpha rhythm, show that the personal questions were most effective in altering the rhythm. Such a result was not unexpected. But the impersonal questions given in random order with the personal questions were next in effectiveness. This was probably due to the fact that the subject was in an anticipatory state, since it was not always possible for him to determine the nature of the question before it was completed.

It should be observed that in all of the experiments reported here the stimuli were given through only the auditory sense modality. Other investigators have shown that auditory stimuli are not consistently effective in altering brain potential patterns (9; 3). Had stimuli with similar affective values been offered through other sense modalities, particularly the visual, it is likely that much different results would have been obtained.

Summary and Conclusions

The purpose of this study was to determine the possible correlation between the patterning of brain potentials and mild affective states.

Five experienced judges independently compared brain-wave records from thirty-three subjects for the purpose of locating those records taken when lies were being told. Ninety lying situations were used. The criterion used in judging was that of the general alteration of the alpha rhythm. The lie records were recognized 130 per cent more often than could be expected from chance. The chi squared value indicated that such a performance could not be expected once in 100,000 times. Records taken during the giving of key words based upon a guilt situation permitted the detection of guilt with about the same degree of accuracy. The brain potential records of the various subjects varied widely in affording clues that would allow consistent recognition of the lie or guilt record. The judges varied in ability to recognize the correct record.

A free-association list of fifty words designed to contain words of pleasant and unpleasant affect was given to twelve subjects, each person being directed to respond with the first word that occurred to him after the stimulus word was given. About twenty seconds later an affective rating of the stimulus was

requested from nine of the subjects. Statistical treatment of the data disclosed no consistent relationships among reaction time, latency, perseveration time, and ratings. "Good" alpha rhythms were disturbed least by the stimulus words.

Berger rhythm records were taken from ten subjects while they listened to ten unemotional statements, five true and five false; ten sentences, five of which were statements and five questions; and ten questions, five personal and five impersonal. The mean percentage of the time the alpha rhythm was out or markedly depressed was calculated for each of the groups of five sentences. No significant differences were found among the means of these various groups for the first five seconds, the second five seconds, or the whole ten seconds after the sentences were completed. Personal questions were significantly more effective in obliterating the alpha rhythm before the completion of the sentences than were any other type of sentences.

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THE EFFECT OF BENZEDRINE AND CAFFEINE UPON PERFORMANCE IN CERTAIN PSYCHOMOTOR TASKS *

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A NUMBER of authors have reported that benzedrine decreases the feeling of fatigue, benefits sufferers from chronic exhaustion, increases energy and capacity for work, as well as motor activity.[1] These reports have been based upon clinical observations and upon the subjective reports of patients and subjects.[2] When the present study was begun, no one had attempted a carefully controlled and objective study of the problem. It seemed desirable, therefore, to determine the influence of benzedrine sulfate upon the performance of normal subjects in certain psychomotor tasks.

Tasks were chosen to represent various aspects of psychomotor activity: speed of making responses (three reaction-time tests), capacity for muscular work (two handgrip tests), speed of movement (tapping tests), and steadiness.

* The authors wish to express their gratitude to Miss Jane Medlar and Mr. Frank Dudek, who acted as subjects for this long and exacting experiment. They wish also to acknowledge their indebtedness to Jerry Thornton for assistance in the laborious task of reading records. Dr. Holck and Mr. Smith are from the Department of Physiology and Pharmacology, College of Pharmacy; Mr. Thornton from the Psychological Laboratory.

[1] The following are representative of these reports:

E. Guttmann. The effect of benzedrine on depressive states. *J. ment. Sci.*, 1936, **82**, 618–620. "One sees in normal persons that they move faster, work quicker when under the effect of the drug" (619).

M. H. Nathanson. The central action of beta-aminopropylbenzene (benzedrine). *J. Amer. med. Ass.*, 1937, **108**, 528–531. Many of Nathanson's subjects reported they felt increased energy, desire and capacity for work, also less fatigue.

P. Solomon, R. Mitchell, and M. Prinzmetal. The use of benzedrine in postencephalitic Parkinson's disease. *J. Amer. med. Ass.*, 1937, **108**, 1765–1770. The authors, observing 26 postencephalitic patients, noted that 96 per cent showed improvement in "energy and well-being" and 73 per cent in "strength."

E. Davidoff, and E. C. Reifenstein, Jr. The stimulating action of benzedrine sulphate, a comparative study of the responses of normal persons and of depressed patients. *J. Amer. med. Ass.*, 1937, **108**, 1770–1776. The authors, observing ten normal subjects, reported that under benzedrine all ten showed increased motor activity and increased general efficiency. Many patients who had complained of fatigue showed amelioration under benzedrine medication.

[2] For a good general review of benzedrine see: Present status of benzedrine sulfate. *J. Amer. med. Ass.*, 1937, **109**, 2064–2069 (a report from the Council on Pharmacy and Chemistry, P. N. Leech, secretary).

Since the previous reports suggested that benzedrine is a stimulant, there was included in the study a drug known to have a stimulating effect upon psychomotor activities in order to make possible direct comparison under comparable conditions of the effects of benzedrine and the known stimulant. The stimulant chosen for this purpose was caffeine.

Three persons served as subjects. One (S_3), a male graduate student in pharmacology, was the third author. This subject knew what drugs were being studied; he had never taken benzedrine, however, and had no means of identifying the drugs during the experiment except by his reactions to them. The other two subjects were completely ignorant as to what drugs were given them. S_1 was an undergraduate male student, age 19; S_2 was a young woman, a graduate student in psychology. Since the results for the three subjects do not differ in their general trend, it seems fair to assume that what knowledge S_3 had did not distort his results to any important degree.

PROCEDURE

The general plan of the experiment was to obtain for each of three subjects measures of his performance on the psychomotor tasks for an equal number of days under each of three drug conditions—normal (no drug), benzedrine, and caffeine—with all other conditions so controlled or counterbalanced that the results for the three drug conditions would be directly comparable.

Control of Conditions. On every experimental day each subject took a capsule with a glass of water one hour and twenty-five minutes before coming to the experimental room.[3] The capsules were of three kinds, containing, respectively, 300 mg. of caffeine sodium benzoate, 20 mg. benzedrine sulfate, and lactose only.[4] These will be referred to as caffeine, benzedrine, and blank capsules;

[3] Cheney found that caffeine had maximum effect upon reaction time from 90 to 120 minutes after administration and only slightly less effect 150 minutes after. (R. H. Cheney. Reaction time after caffeine and coffee consumption. *J. exper. Psychol.*, 1936, **19**, 357–369.) Reports concerning the onset of effects of benzedrine have varied. R. W. Webb, Director of Research at the Smith, Kline, and French Laboratories, in personal correspondence has stated the opinion that the effects are felt by most individuals within 15 minutes, reach a peak in one to two hours, and then gradually decline. The experimental tests were conducted, therefore, between 90 and 150 minutes after administration of the drug, when the effects of either drug were probably at or near the maximum.

[4] The writers wish to thank Smith, Kline, and French Laboratories, Philadelphia, who generously supplied all the drugs used in the experiment and also prepared all capsules, making them so much alike that no one could tell them apart. After the article was accepted for publication, this company made a small grant to cover part of the cost of publication, for which we are also grateful.

and the days on which they were taken will be called, respectively, caffeine, benzedrine, and normal days. All capsules were made identical in appearance and taste, and neither the subject nor the experimenter had any means of knowing what drug had been administered except by observing the effects.

Every effort was made to make every experimental day, except for the drug administered, the same as every other for the given subject. All experimental sessions were in the same room, in which the lighting was artificial and the temperature thermostatically controlled (75°F). The subjects were tested individually with the same experimenter throughout. The program for each experimental session was the same. Monday, Wednesday, and Friday were chosen as experimental days because the daily program for each subject was most nearly alike on these days. Each subject had a set hour for his experimental session which was the same for every day of the first and last halves of the experiment, respectively.

In order to minimize practice effects, each subject was given eight practice sessions. After the third practice session he was not told his scores or permitted to see the records. During the practice sessions each subject was instructed to develop the best technique he could for each task and to be alert to ways of improving his performance. Typed and oral instructions emphasized that the schedule at each experimental session would be exactly the same and that it would be extremely important that the subject should use the same techniques and maintain the same attitude toward the tests at every session, particularly that he should do his very best in every test every day.

The subjects were requested to keep their schedules for each 24 hours preceding an experimental session as constant as possible as to bed-time, hours of sleep, breakfast hour and menu, and general activities—especially to avoid strenuous activity just prior to the test session. The subjects were further required to abstain from coffee, tea, tobacco, alcoholic beverages, and other drugs. Each subject apparently made a conscientious effort to follow these instructions.

The experimental sessions began on November 29, 1937 and ended on March 12, 1938. No sessions were held during the two weeks of Christmas vacation and the one week of semester examinations. These came after the third and sixth weeks, respectively, of the experiment. All records were scored at the end of the experiment without knowledge as to which records were for caffeine, benzedrine, or normal days.

Counterbalanced order. In order to equalize any practice effects or other conditions which were not otherwise controlled, the order for the administration of drugs on successive experimental days was as follows: [5]

S_1: ABCXBA BCAACB CABBAC CABBAC BCAACB ABCCBA
S_2: BCAACB CABBAC ABCCBA ABCCBA
S_3: CABBAC ABCCBA BCAACB BXAACB AXCCBA CABBAC

As may be seen from the above order, the counterbalancing is not quite complete in that S_2, who was taken ill, completed only 24 of the intended 36 sessions; and one day's record for S_1 and two days' records for S_3 had to be dropped because unavoidable circumstances made the conditions on those days not comparable. Also the tapping records for the third through the sixth weeks

[5] *A* is caffeine, *B* is blank, *C* is benzedrine; *X* signifies that the experiment was taken but the record has not been included in the results.

for S_8 were dropped from the results. This omission was necessary because S_3's technique in the tapping tests became faulty during the fourth and fifth weeks. Whenever it was decided to drop a record, the decision was made without knowledge as to what drug had been administered.

With the above stated exceptions, which do not seriously modify the following statements, the counterbalanced order accomplished these ends:

(1) For each subject the scores for the various drug conditions are directly comparable for successive two-week periods. (Practice effects are adequately counterbalanced.)

(2) Each subject received each drug the same number of times on Monday as on Wednesday and as on Friday.

(3) The order of administration throughout the experiment for any one drug for S_1 was the same as that for a second drug for S_2 and for a third drug for S_3.

(4) On any given experimental day each subject received a different drug. Thus, any effect that the day—weather conditions, school program, mood of the experimenter—might have upon the performance was counterbalanced between subjects.

The Tests. Each experimental session for every subject followed this schedule:

5 minutes of rest.
5 minutes to take temperature.
Steadiness test: 2 trials with 1-minute rest between.
Reaction-time tests:
    Simple auditory with two-second preparatory interval.
    Simple auditory with indefinite preparatory interval.
    Visual choice
Tapping tests:
    30-second trial, followed by 30-second rest.
    2-minute trial, followed by 2-minute rest.
    5-minute trial.
1-minute rest.
Handgrip: 5 trials—one every 30 seconds.
1-minute rest.
Maintained grip: 2 trials with 1-minute rest between.
1-minute rest.
Steadiness test: 2 trials with 1 minute rest between.

In the steadiness test the Stoelting steadiness tester was used. The apparatus was placed at approximately shoulder height (21.2 cms. above the table top) as the subject was seated before it. In each trial the subject was required to hold the metal stylus in the smallest hole of the tester (3 mm. in diameter) for one minute. He was given five seconds to adjust the stylus in place before the beginning of each trial. The score for the trial was the number of times he made contact with the side plus a correction for continued contacts. The score for the day consisted of the total for the four trials. The apparatus was wired to a signal magnet and every contact made recorded on a moving kymograph drum. A second writing point registered on the drum the beginning and end of each trial. A third writing point in circuit with a pendulum marked off a time line in seconds. The recording apparatus and the experimenter were hidden from the subject's view.

In the reaction-time tests the signals were electrical, initiated by the experimenter. For the simple auditory with preparatory interval (hereafter designated as A.R.T.), the signal was a click made by an electrical sounder. This was preceded by a spoken "Ready" given approximately two seconds before. The time of the preparatory interval was measured by means of a large pendulum which made one double vibration in two seconds. Twenty-one trials were given at approximately twenty-second intervals. The median time was taken as the score for the day.[6]

The simple auditory reaction time with indefinite preparatory interval (hereafter designated I.R.T.), was conducted in the same manner as the above A.R.T. with the exception that the click from the electrical sounder came at intervals varying from one to ten seconds after the spoken "Ready." The timing of the preparatory interval again was by means of the pendulum. In each session there were twenty-one trials, the score for the session being the medium time for these twenty-one trials. Six different mixed-up orders of preparatory intervals were arranged, and each order was used on one day of each two-week period. The order for using these six arrangements was varied during the course of the experiment to prevent learning and to counterbalance the effects in relation to drugs taken.

In the visual-choice reaction time (hereafter designated C.R.T.), the signal was one of a pair of lights placed 4 cms. apart on the same level at about the height of the subject's eyes and approximately 60 cms. distant from his eyes. These lights were centered on a black cardboard background 22.5 by 32 cms., which in turn was on a light gray background. The lights were from 2.5-volt Mazda bulbs which obtained current from three 1.5-volt dry cells wired in series. A two-second preparatory signal was given as in the A.R.T. The subject responded by pressing a key with his right hand for the right light and with his left hand for the left light. The order of presentation of right and left signals was "random" following an arranged schedule. Six "random" schedules were prepared. Each schedule included, respectively, 9, 12, 10, 11, 9, 12 right-hand trials. Every schedule was used on one day of every two-week period. The order for the use of schedules was varied to prevent learning and in such a way that each pair of caffeine, benzedrine, and normal days in each two-week period had a total of 21 right-hand trials and 21 left-hand trials. The score for each day was the median reaction time for that day. When a subject responded with the wrong hand, a signal light informed the experimenter. The time was recorded, but the trial was not counted in the results.[7] Additional trials, when needed, were given at the end of the series without informing the subject.

In all of the reaction-time tests the subject made his response by pressing a telegraph key. In each case his fingers were placed on the key before the signal was given, with instructions to be ready to press the key the instant the signal was given. The responses for A.R.T. and I.R.T. were all made with the right hand. For the C.R.T. two keys were used, one for each hand. The response keys were wired into an electrical circuit which stopped a Bergstrom chronoscope. The pendulum of the chronoscope was started by means of the circuit by which

[6] The median score for each day is better than the mean score, because the mean can be too much affected by a few extremely slow scores.

[7] The number of errors was too small (less than one per day) to indicate reliably whether the drugs tended to increase the errors in C.R.T. Every subject, however, had more errors on benzedrine days. The average number of errors per test for all subjects was 1.1 on benzedrine days, .7 on caffeine days, and .5 on normal days.

the signal (the click from the sounder or the flash of the light) was controlled. Thus the chronoscope was started at the precise instant the signal was given and was stopped at the instant the subject's response was made. No attempt was made to measure lag in the chronoscope; if present, the lag was a constant error and would not affect materially our comparisons.

The experimenter and the apparatus for the reaction-time tests were in the same room as the subject but were hidden from his view; only the response keys and the C.R.T. signal lights were visible to the subject. The chronoscope in starting made a slight click which was audible to the subject. This and perhaps other accessory sounds probably formed a part of the sound complex which made up the signal for the A.R.T. and I.R.T. tests. In the C.R.T. tests the accessory sounds probably constituted an additional preparatory signal for the subject. A more serious difficulty in the A.R.T. was the apparent tendency for the subject occasionally to anticipate the sound stimulus and to respond instead to the estimated time interval. What effect this circumstance had on the results is not known. It is possible, also, that in the A.R.T. and I.R.T. tests the subjects took auditory cues from unavoidable slight movements of the experimenter. Furthermore, since the room was not sound-proof, noises in the building and outside sometimes acted as distractors. These factors probably increased the variability of the results but did not introduce any constant errors.

The apparatus used for the tapping test consisted of two copper plates each three inches square mounted one inch apart and separated by a rounded block raised approximately one-half inch above the surface of the plates. The subject was required to tap alternately on the plates with a metal stylus. The apparatus was hooked into circuit with writing points so that each tap was recorded on a moving kymograph drum, the taps on the right-hand plate by one writing point and the taps on the left-hand plate by a second writing point. A third writing point, hooked into circuit with a pendulum, recorded the time in seconds. Each subject was permitted to use his own technique for tapping but was urged to tap as fast as he could right from the start and not to save himself for the last part of the longer trials.

For the handgrip test the modified form of the Smedley dynamometer was used. The stirrup was set at exactly the same point on every day for each subject. Only the right hand was used. Trials were made every thirty seconds. The score for each day was the total in kilograms for five trials.

For the maintained handgrip test a marker was set at a given point on the dial of the dynamometer for each subject. This point was two-thirds of the average handgrip which the subject showed in the first three practice sessions. The subject was required to hold the pointer of the dynamometer above the marker as long as possible. The score for the day was the total number of seconds, measured by a stop-watch, that the subject kept the pointer above the marker in two trials.

RESULTS [8]

Comparison of Scores under Benzedrine and Normal Conditions. Table 1 presents the scores for each of the three subjects for all the tasks under each drug condition. Each score repre-

[8] In the calculation of results, the figures were carried to more decimal places than are found in the following tables.

sents an average of all trials on the given task for the given subject and drug. It may be observed that in every task every subject did better on the benzedrine days than on the normal (blank) days. The tasks which show the greatest difference in favor of benzedrine days are the steadiness and maintained handgrip. The reaction-time tests show only a slight difference in favor of the benzedrine days. The amount of difference varies

TABLE 1

AVERAGE SCORES FOR EACH OF THREE SUBJECTS ON NINE PSYCHOMOTOR TASKS UNDER CAFFEINE (C), BLANK (Bl), AND BENZEDRINE (B) CONDITIONS

TASK [9]	SUBJECT 1			SUBJECT 2			SUBJECT 3		
	C	Bl	B	C	Bl	B	C	Bl	B
Stead.	53.2	43.4	25.5	87.9	72.8	30.9	76.4	72.3	37.9
A.R.T.	119.3	118.5	112.3	134.8	132.4	130.3	133.8	146.8	138.5
I.R.T.	144.8	145.9	137.3	146.8	151.5	144.4	184.4	211.3	198.8
C.R.T.	211.4	216.0	210.3	219.5	222.3	214.0	256.8	274.4	268.7
30″ tap	246.9	232.9	268.0	186.3	178.3	195.0	194.3	182.7	196.1
2′ tap	883.7	840.5	932.8	674.4	650.3	723.3	748.4	647.0	757.4
5′ tap	668.8	642.7	719.3	499.5	482.0	543.1	563.5	492.7	589.6
Grip	247.7	243.8	248.8	178.8	171.8	185.8	280.5	261.0	284.0
M. grip	68.7	65.4	71.0	28.9	22.3	49.3	54.9	42.0	66.5

not only with the task but also with the individual subject. These facts may be seen readily in Table 2, which presents the percentage of improvement of scores on benzedrine days as compared with normal days.

How reliable are these differences? The fact that all three subjects agree in the direction of the results for all tasks suggests strongly that there is a real difference in favor of the benzedrine days. It is extremely unlikely that in all 27 cases (nine tasks each for three subjects) the differences would favor benzedrine days because of sampling errors.

[9] The abbreviations in the task column stand for the following tests, respectively: steadiness, simple auditory reaction time, indefinite reaction time, choice reaction time, 30-second tapping, 2-minute tapping, 5-minute tapping (first, middle, and last half minutes only), handgrip, maintained handgrip. The above abbreviations will be used in the tables to follow.

When individual tasks are considered, the differences between benzedrine and normal days seem to be reliably established in some cases but not in others. Table 4 presents the differences, each one with its standard error of the difference and the critical ratio. The critical ratios are given in terms of sigmas. Any difference yielding a critical ratio above 2.0, therefore, is probably in the true direction. In calculating the standard errors of the means upon which the standard errors of the differences

TABLE 2

PERCENTAGE OF IMPROVEMENT OF SCORES UNDER BENZEDRINE AND CAFFEINE CONDITIONS OVER SCORES UNDER NORMAL CONDITIONS FOR EACH OF THREE SUBJECTS ON NINE TASKS[10]

TASK	BENZEDRINE			CAFFEINE		
	S_1	S_2	S_3	S_1	S_2	S_3
Stead.	41	58	48	—23	—21	—6
A.R.T.	5	2	6	—1	—2	9
I.R.T.	6	5	6	1	3	13
C.R.T.	3	4	2	2	1	6
30″ tap	15	9	7	6	5	6
2′ tap	11	11	17	5	4	16
5′ tap	12	13	20	4	4	14
Grip	2	8	9	2	4	8
M. grip	9	121	58	5	30	31

are based, the conservative formulae were used which make corrections for the small number of cases involved.[11] Because the correlations were based upon so few cases, in most instances in calculating the sigma of the difference the correlation was assumed to be zero. In those cases which are starred in Table 4, there was such obvious change in the general level of the scores as the experiment progressed that the sigmas of the differences

[10] Each figure is a percentage found by dividing the difference between benzedrine or caffeine score and the normal score by the normal score. The scores referred to are the average scores from Table 1. Improvement in the first four tasks means reduction of scores and cannot be more than 100 per cent, whereas improvement in the last five tasks can be more than 100 per cent. This affects notably comparisons involving "Stead." and "M. grip."

[11] J. P. Guilford. *Psychometric methods*. New York: McGraw-Hill, 1936, p. 51.

were unduly increased.[12] In these few cases the correlations between the weekly scores for the different drug conditions were taken into account in the calculation of the sigmas of the differences. What the critical ratios would be if the assumption of zero correlation were made, is given in a footnote to each table.

To summarize the evidence on reliability found in Table 4, the differences in favor of benzedrine seem quite certainly established for all three subjects in the three tapping tests and in the steadiness test, and for two of the subjects in the handgrip and maintained handgrip tests. In the reaction-time tests, however, the differences cannot be regarded as surely established. Of these latter, the difference for the A.R.T. is least reliably established.

A third bit of evidence concerning the reliability of the above differences agrees in general with the evidence presented in Table 4. This is the evidence which may be had by comparison of the averages for successive two-week periods of the experiment, presented in Table 3. If the differences in scores between benzedrine and normal days were due only to sampling errors, it would follow that in approximately half of the cases in Table 3 the benzedrine scores would be better and in approximately half of the cases the normal scores would be better. Such is obviously not the case. In none of the tapping tests does the normal score ever equal or exceed the comparable benzedrine score. In the steadiness test and the handgrip in only one of sixteen cases and in the maintained handgrip in only two of sixteen cases do the normal scores exceed the comparable benzedrine scores. In the reaction time tests the differences are more often in favor of the benzedrine scores but less regularly so than in the previously mentioned tests.

Comparison of Caffeine and Normal Days. As may be seen from Tables 1 and 2, the average scores on caffeine days in comparison with normal days are poorer for the steadiness test, doubtful for the A.R.T., and better for all the other tasks studied. The degree of change in the score as compared with normal days varies with the task and the individual. (See Table 2.)

[12] In the three tapping tests for S_1, the score increased with marked regularity from the fourth to the twelfth weeks. The scores on the twelfth week averaged 60 per cent better than those for the first week.

TABLE 3

AVERAGE SCORES FOR SUCCESSIVE TWO-WEEK PERIODS FOR EACH OF THREE SUBJECTS ON NINE PSYCHOMOTOR TASKS UNDER CAFFEINE (C), BLANK (BL), AND BENZEDRINE (B) CONDITIONS

TASK[13]	SUBJECT 1			SUBJECT 2			SUBJECT 3		
	C	Bl	B	C	Bl	B	C	Bl	B
STEAD.									
1st	37	38	22	84	76	28	102	94	65
2nd	59	48	23	98	48	25	128	90	43
3rd	45	46	26	84	68	21	64	65	14
4th	30	64	28	86	100	51	53	46	28
5th	77	41	26				49	113	35
6th	72	25	27				64	47	39
A.R.T.									
1st	130	136	118	140	141	135	139	141	131
2nd	148	133	145	141	131	136	154	154	160
3rd	111	113	108	131	132	127	134	161	134
4th	111	109	104	128	127	124	127	154	131
5th	110	112	99				122	158	133
6th	107	110	105				129	120	140
I.R.T.									
1st	152	160	151	149	156	145	194	205	183
2nd	153	149	147	136	148	144	199	212	210
3rd	133	143	127	155	149	147	186	211	197
4th	141	140	130	148	154	142	177	227	212
5th	152	150	144				170	220	200
6th	139	135	133				183	198	198
C.R.T.									
1st	206	235	208	221	230	211	242	263	243
2nd	224	222	219	211	211	211	261	285	262
3rd	212	201	205	218	223	213	253	277	278
4th	216	215	215	228	226	222	266	283	279
5th	224	204	208				258	280	278
6th	187	220	206				262	263	279
30″ TAP									
1st	201	191	214	201	182	199	202	193	201
2nd	187	188	223	185	178	199			
3rd	225	213	224	181	175	191			
4th	266	232	276	179	180	191	189	176	187
5th	289	287	308				190	173	196
6th	314	288	337				197	184	197
2′ TAP									
1st	711	683	768	691	665	717	795	730	809
2nd	661	681	764	678	649	715			
3rd	783	754	839	673	646	713			
4th	942	875	966	656	642	750	733	587	737
5th	1069	1009	1016				740	558	729
6th	1137	1042	1163				727	669	746
5′ TAP									
1st	516	517	578	498	489	529	631	583	654
2nd	518	512	550	501	484	537			
3rd	603	594	658	496	484	548			
4th	724	661	754	504	472	560	521	428	575
5th	814	784	792				554	420	544
6th	840	789	914				549	504	579
GRIP									
1st	250	250	256	178	174	183	265	253	266
2nd	256	245	261	175	171	184	264	235	266
3rd	249	244	242	179	167	184	273	272	293
4th	240	243	250	184	176	194	291	271	311
5th	249	239	246				298	265	297
6th	242	242	242				293	273	284
M. GRIP									
1st	72	67	76	46	30	71	64	50	66
2nd	71	67	77	21	28	52	59	43	64
3rd	71	73	71	26	17	42	52	41	79
4th	64	59	74	23	14	33	55	51	73
5th	72	56	67				51	33	68
6th	62	70	65				49	30	52

[13] The *1st, 2nd,* etc. in the *Task* column refer to the successive two-week periods.

In general, the maintained handgrip and the steadiness test seem to be affected most and the reaction times least.

The amount of these differences and their sigmas and the critical ratios are presented in Table 4. Although 12 out of 27

TABLE 4

DIFFERENCES BETWEEN THE AVERAGE SCORES UNDER BENZEDRINE (B), BLANK (Bl), AND CAFFEINE (C) CONDITIONS, SIGMAS OF THESE DIFFERENCES, AND CRITICAL RATIOS (CR)[14]

TASK	B–Bl			C–Bl			B–C		
	DIFF.	$\sigma_{diff.}$	CR	DIFF.	$\sigma_{diff.}$	CR	DIFF.	$\sigma_{diff.}$	CR
SUBJECT 1									
Stead.	18.0	7.1	2.6	—9.8	9.7	1.0	27.7	8.1	3.4
A.R.T.	6.2	6.6	.9	—.8	6.7	.1	7.1	7.9	.9
I.R.T.	8.7	4.6	1.9	1.2	4.3	.3	7.5	4.2	1.8
C.R.T.	5.8	6.7	.9	4.6	7.6	.6	1.2	7.0	.2
30″ tap	35.1	10.1*	3.5	14.0	5.6*	2.5	21.1	6.9*	3.1
2′ tap	92.3	20.8*	4.5	43.2	18.0*	2.4	49.2	20.8*	2.4
5′ tap	76.6	21.5*	3.6	26.2	10.4*	2.5	50.4	14.0*	3.6
Grip	5.1	2.6*	2.0	4.0	2.4*	1.6	1.1	2.3*	.5
M. grip	5.6	3.9	1.4	3.3	2.8	1.2	2.4	3.9	.6
SUBJECT 2									
Stead.	41.9	11.5	3.6	—15.1	13.9	1.1	57.0	11.1	5.1
A.R.T.	2.1	5.0	.4	—2.4	3.8	.6	4.5	5.0	.9
I.R.T.	7.1	5.0	1.4	4.8	5.7	.8	2.4	6.4	.4
C.R.T.	8.3	5.6	1.5	2.8	7.2	.4	5.5	5.8	1.0
30″ tap	16.8	3.1	5.4	8.0	4.4	1.8	8.8	4.6	1.9
2′ tap	73.0	10.6	6.9	24.1	11.1	2.2	48.9	11.7	4.2
5′ tap	61.1	8.0	7.7	17.5	7.3	2.4	43.6	8.1	5.4
Grip	14.0	3.0	4.6	7.0	2.8	2.5	7.0	3.1	2.3
M. grip	27.0	7.6	3.5	6.7	6.3	1.1	20.4	8.8	2.3
SUBJECT 3									
Stead.	34.4	11.8	2.9	—4.2	15.0	.3	38.5	13.6	2.8
A.R.T.	8.4	8.8	1.0	13.0	8.4	1.5	—4.6	6.5	.7
I.R.T.	12.5	7.4	1.7	26.9	6.6	4.1	—14.4	7.4	1.9
C.R.T.	5.7	6.7	.9	17.5	5.5	3.2	—11.9	6.0	2.0
30″ tap	13.4	4.8	2.8	11.5	5.2	2.2	1.9	4.4	.4
2′ tap	110.4	42.7	2.6	101.4	37.3	2.7	9.1	31.1	.3
5′ tap	96.9	42.5	2.3	70.8	40.5	1.8	26.1	33.2	.8
Grip	23.0	6.0*	3.9	19.5	4.7*	4.2	3.4	3.5*	1.0
M. grip	24.4	5.3	4.6	12.8	3.9	3.3	11.6	4.8	2.4

of the differences appear to be determined with a fair degree of certainty, in most cases the statistical reliability of these differences is less than that found for benzedrine and normal days.

[14] All sigmas of the differences were calculated on the assumption of zero correlation except cases that are starred. (See text.) If a like assumption is made in the cases starred, the critical ratios become, in the order of their appearance in the table—for the *B–Bl* column: 1.7, 1.4, 1.3, 1.7, and 3.0; for the *C–Bl* column: .7, .6, .5, 1.6, and 2.7; for the *B–C* column: 1.0, .6, .8, .3, and .5.

The evidence from comparison of the comparable biweekly caffeine and normal scores (see Table 3) agrees fairly well with the critical ratios presented in Table 4. For the tapping tests the caffeine scores excel the comparable normal scores in 42 out of 46 cases. The steadiness scores for caffeine are poorer than the comparable normal scores in 10 out of 16 cases. In both the handgrip and maintained handgrip tests the normal scores equal or excel the comparable caffeine scores in 3 out of 16 cases.

The results under caffeine medication reported in this study are in harmony with the findings of previous experimenters. No conclusive evidence has been presented previously for reaction-time tests which exactly parallel our A.R.T. and I.R.T. Early investigations yielded conflicting evidence, but most of the experiments were insufficiently controlled. A recent and better controlled experiment has indicated a decrease under caffeine of simple visual reaction time.[15] The present findings in the C.R.T. agree with those of Hollingworth,[16] whose test was closely similar, and with the recent findings of Cheney.[17] The results of Hollingworth[18] and Hull[19] have indicated that caffeine in doses similar to that of this study causes increase in unsteadiness. Early experiments with the dynamometer yielded evidence that caffeine increases the force of muscular contractions.[20] While no evidence has been presented on a task exactly paralleling the maintained handgrip, a whole group of ergographic experiments have indicated that caffeine stimulates the capacity for muscular work.[21] In a tapping test somewhat similar to the present ones Hollingworth found that caffeine increased the speed of tapping.[22] Thus, where comparable evidence is available, the results of the present study agree with the findings of previous investigators.[23] This fact suggests that the methods of

[15] K. Horst and W. L. Jenkins. The effect of caffeine, coffee, and decaffeinated coffee upon blood-pressure, pulse rate and simple reaction time of men of various ages. *J. Pharm. exper. Therapy*, 1935, **53**, 385–400.

[16] H. L. Hollingworth. The influence of caffeine on mental and motor efficiency. *Arch. Psychol.*, No. 22, 1912. See Chapter 11.

[17] R. H. Cheney, *op. cit.*

[18] Hollingworth, *op. cit.*, Chap. 5.

[19] C. L. Hull. The influence of caffeine and other factors on certain phenomena of rote learning. *J. gen. Psychol.*, 1935, **13**, 249–274.

[20] See Hollingworth, *op. cit.*, p. 1, for a summary of these experiments.

[21] Hollingworth, *op. cit.*, summarizes ten such studies. Cf. pp. 1 f.

[22] Hollingworth, *op. cit.*, Ch. 4.

[23] In the above comparisons the size of the dose of caffeine has been taken into account, the evidence cited being that for doses most comparable to the dose used in this study.

investigation used in the present study were adequate and that the subjects tested were typical in their reaction to a stimulant.

Comparison of Benzedrine and Caffeine Days. The facts presented in Tables 1 and 2 indicate that the influence of benzedrine and caffeine in the doses used is in the same direction in all tasks except the steadiness test and the A.R.T. In the A.R.T. the effect of caffeine upon the performance has not been established with any certainty. In the steadiness test, however, it appears likely that the effects of caffeine and benzedrine in the doses used are in the opposite direction. In the tapping tests, the handgrip, and the maintained handgrip the scores on benzedrine days are better than the scores on caffeine days. In the reaction-time tests for S_1 and S_2 the scores on benzedrine days are better, but for S_3 the scores are better on caffeine days. It is probable, therefore, that with the exception of the reaction-time tests for S_3 the 20 mg. dose of benzedrine for the given subjects had more stimulating effect upon the psychomotor performances studied than had the 300 mg. dose of caffeine. The degree of reliability of the differences between caffeine and benzedrine scores may be seen from the critical ratios given in Table 4, and by comparison of the biweekly scores in Table 3.

As noted above, the differences between benzedrine and normal scores are in general more reliably established than are the differences between caffeine and normal scores (Table 4). Since the results for caffeine are in agreement with those previously reported and since caffeine was studied under the same conditions as benzedrine, this tends to confirm the evidence that benzedrine has a beneficial effect upon at least some of the tasks studied.

Discussion. It is interesting to speculate concerning the causes of the variability of effect of benzedrine and caffeine upon the different tasks studied in this experiment. One might expect that those tasks which are affected most by slight changes in experimental conditions would be affected most by the drugs. This hypothesis is supported by the fact that there is some relationship in the present data between the degree of improvement of the scores after administration of the drugs and the coefficient of variation of the daily scores under normal conditions. The Pearson correlation between the percentage of improvement

under benzedrine (as in Table 2) and the coefficients of variation of the scores under normal conditions is .74±.06. A similar correlation between the percentage of improvement under caffeine and the coefficients of variation is .61±.08. These coefficients may be interpreted as indicating that in those tasks in which slight variations in experimental conditions cause relatively large variations in performance, caffeine or benzedrine tend to have a relatively large effect upon the performance.

It would be even more interesting if a similar relationship were found between the variability of the performance of individual subjects and the degree to which the drugs affect the subjects. It would also be of interest to know how the degree to which drugs affect subjects is related to differences in sex, age, and marital or sexual status. The meager data obtained in this study, however, do not warrant even tentative conclusions on these points.

The results presented above establish the fact that benzedrine has a stimulating effect upon certain psychomotor performances. They yield less satisfactory information concerning how this stimulation comes about. The spontaneous remarks of the subjects during the course of the experiment, as well as numerous reports in the literature, indicate that benzedrine causes a feeling of well-being and a sense of increased capacity for work. It seems most likely that these effects are accompaniments of the psychomotor stimulation. It is possible, however, that these experiential effects are causally antecedent to the psychomotor stimulation, i.e., that the subjects do better on the psychomotor tasks *because* they " feel " better—through a process akin to autosuggestion. The present results do not discriminate between these two possibilities.

It is possible that during the course of the experiment, learning plus suggestion may have entered in to augment the differences in favor of benzedrine days. Perhaps the subjects came to associate the sensations occurring on benzedrine days with improvement of scores and so came to expect more of themselves on days when they had these sensations.[24] It is not clear, however, that such an expectation would cause improvement of

[24] The two tests in which the subjects showed the greatest improvement, steadiness and maintained handgrip, would seem to permit the subjects to estimate their scores more accurately. Whether this fact is significant or merely coincidental is not apparent.

scores; and the figures in Table 3 do not show a consistent increase in the differences in favor of benzedrine as the experiment progressed. Since the differences in favor of benzedrine were similar in earlier and later parts of the experiment, these differences cannot be attributed principally to the influence of learning.

The facts in Table 3 do not give any clear indication of habituation to benzedrine in the course of the experiment. The biweekly scores, it is true, are not reliably enough determined to yield evidence concerning possible small differences due to habituation. It is possible also that habituation tended to decrease somewhat the effects of the drug; but learning, as discussed in the preceding paragraph, offset this tendency. The conclusion is probably warranted, however, that there was no marked habituation in the present experiment.

It has been reported in the literature (cf. footnote 1) that one of the effects of benzedrine is to diminish and relieve fatigue. These reports suggest the possibility that the improvement of scores on benzedrine days is due to a decrease in the subject's sensitivity to fatigue. Parts of the experiment were set up with this hypothesis in mind: three tapping tests of differing length were used; the tests in the later part of the experimental session were crowded together, the rest period after the five-minute tapping test and after each succeeding test being only one minute; the scores on several of the tests were recorded in such a manner that the performance on the first part of the test could be compared with the performance on the last part of the same test. A measure of the fatigue effects was obtained by computing the ratios of (1) the scores on the shorter tapping tests to the scores on the longer tapping tests, (2) the scores on the last two trials to the scores on the first two trials of the steadiness test,[25] (3) the scores on the first part of certain tests to the scores on the last part of these tests. These ratios are given in Table 5. In each case the larger the ratio, the greater is the indication of fatigue effects. Hence, if the hypothesis is correct that benzedrine improves the scores by reducing the subject's sensitivity to fatigue, the ratios in Table 5 should be smaller for benzedrine days than for normal days. This is true for S_3

[25] The last two trials of the steadiness test came at the end of the experimental hour; the first two trials came at the beginning of the hour.

but not for S_1 and S_2. Indeed, an opposite hypothesis seems to be indicated for S_1. The evidence concerning fatigue effects on caffeine days as compared with normal days parallels that for fatigue effects on benzedrine days—that is, S_1 appears to show more fatigue effects on caffeine days, S_2 shows about the

TABLE 5

EVIDENCE ON FATIGUE EFFECTS: RATIOS OF SCORES ON LONGER TRIALS TO SCORES ON SHORTER TRIALS AND RATIOS OF SCORES ON LAST PART OF TRIALS TO SCORES ON FIRST PART FOR CAFFEINE (C), BLANK (BL), AND BENZEDRINE (B) DAYS [26]

RATIO [27]	SUBJECT 1			SUBJECT 2			SUBJECT 3		
	C	Bl	B	C	Bl	B	C	Bl	B
A	1.12	1.11	1.15	1.11	1.10	1.08	1.04	1.13	1.04
B	1.11	1.09	1.11	1.12	1.11	1.08	1.03	1.11	1.00
C	.99	.98	.97	1.01	1.01	1.00	1.00	.99	.96
D	1.79	1.31	1.92	1.16	1.15	1.38	.93	1.25	.52
E	.76	.79	.85	.75	.66	.68	1.00	1.23	1.14
F	1.11	1.07	1.07	1.09	1.09	1.10	1.05	1.15	1.05
G	1.23	1.22	1.28	1.17	1.14	1.15	1.11	1.22	1.03
H	1.03	1.03	1.02	1.02	1.04	1.03	1.01	1.02	1.00
I	1.54	1.52	1.61	.85	1.24	1.21	1.33	1.24	1.59

same amount, and S_3 shows less. The evidence, then, indicates that the effect of benzedrine upon fatigue is similar to that of caffeine. It appears, therefore, that the effect of benzedrine is not a decrease in sensitivity to fatigue but rather a stimulation of psychomotor activity which is in many respects similar to that occurring after caffeine medication.[28]

[26] The larger the ratio the greater the fatigue effects indicated.

[27] The letters in the ratio column represent the following ratios:
A—4 times score on 30″ tap over score on 2′ tap
B—3 times score on 30″ tap over score on 5′ tap
C—¾ of score on 2′ tap over score on 5′ tap
D—score on trials 3 & 4 of steadiness test over score on trials 1 & 2
E—score on last half of each steadiness trial over score on first half of each trial
F—score on first half-minute of 2′ tap over score on last half-minute
G—same as *F* for the 5′ tap
H—score on trials 1 & 2 of handgrip over score on trials 4 & 5
I—score on first trial of m. grip over score on last
(*Score* in each case means average score for whole experiment.)

[28] Two subjects made spontaneous observations that the drug (benzedrine) made them feel like trying harder or putting forth more effort.

The above discussion is not meant to imply that either benzedrine or caffeine might not delay the onset of fatigue in a task where the subject was required to do work at a given rate. In such a task a stronger subject would fatigue less rapidly; likewise, a subject stimulated with a drug might fatigue less rapidly. The present results on the maintained handgrip indicate that such is probably the case.

SUMMARY

Nine psychomotor tasks (including three reaction-time tests, three tapping tests, handgrip, maintained handgrip, and a steadiness test) were studied under three drug conditions. Three subjects had eight practice sessions each and 35, 24, and 34, respectively, experimental sessions. The experimental sessions were alike on all days except that the capsules given the subject, though identical in appearance, on approximately one-third of the days contained 20 mg. of benzedrine sulfate, on one-third 300 mg. caffeine sodium benzoate, and on one-third lactose only. The order of administration of drugs was counterbalanced so that the scores for each subject for benzedrine, caffeine, and normal (lactose) days are directly comparable for two-week periods and for the whole experiment.

The results indicate that in all tasks all three subjects made better average scores on benzedrine days than on normal days. The differences in favor of the benzedrine scores are greatest for the steadiness and maintained handgrip tests and least for the reaction-time tests. Most of the differences are statistically reliable; the differences for the reaction-time tests, however, yield critical ratios (in terms of sigmas) of from .4 to 1.9 only.

In all tests except the steadiness and one reaction time, the average scores for caffeine days are better than for normal days for every subject. The differences for one of the reaction-time tests are variable. In the steadiness test all three subjects did more poorly on caffeine days than on normal days. Where comparable data are available, the present results for caffeine are in harmony with results reported by previous investigators. In general, the statistical reliability of the differences between caffeine and normal days is somewhat less than the reliability of the differences between benzedrine and normal days.

Caffeine and benzedrine affect the performance on the steadi-

ness test in opposite directions, benzedrine improving the performance. In the remaining psychomotor tasks studied, the degree of improvement of scores resulting from the administration of 20 mg. of benzedrine sulfate is in all cases, except the reaction time tests for one subject, greater than the improvement resulting from the administration of 300 mg. of caffeine sodium benzoate. Some of these differences, however, are not reliably established in this experiment.

The effects of benzedrine and caffeine upon the performance of the psychomotor tasks vary in degree both with the subject and with the task. The degree of change in performance shows some positive correlation with the coefficient of variation for the task under normal conditions.

The bearing of the results upon the problem of how benzedrine brings about an improvement in the scores is discussed briefly.

SHORT ARTICLES AND NOTES

WILL HYPNOTIZED PERSONS TRY TO HARM THEMSELVES OR OTHERS?

BY LOYD W. ROWLAND
University of Tulsa

THE purpose of this experiment is to determine the extent to which deeply hypnotized persons (1) will subject themselves to unreasonably dangerous situations and (2) will perform acts unreasonably dangerous to the welfare of others.

It is an old problem about which people have talked a great deal and experimented little. The consensus of opinion in the literature has been that the hypnotized person will *not* violate his own good judgment with respect to possible harm to self or others.[1] In the experiment to be outlined it was decided to examine this commonly accepted hypothesis by means of a new technique made possible by the development of invisible glass.

There are two parts to the experiment. In Part I the problem was to see if hypnotized subjects would expose themselves to danger; in Part II it was the problem to see if they would try to harm others.

PART I

Subjects. Four persons participated in Part I.

Subject A, female, a Junior in the University of Tulsa, preparing to teach in high school.

Subject B, male, co-captain of the University football team, a Senior.

Subject C, female, graduate student, with about twenty years' experience as grade-school teacher.

Subject D, female, about 24 years of age; made frequent visits to the staff of the Department of Psychology for help in the solution of some of her personal problems.

All subjects had been hypnotized at least twice, and there was evidence of deep hypnosis in all cases. There were contractions of muscle groups, amnesias, and hallucinations in all cases.

[1] Representative statements are as follows: Young (4, 511), ". . . it is agreed that the subject will not obey the operator in committing an act which is repugnant to the former's moral scruples." Schilder concludes (3, 33), "If a profoundly hypnotized person is asked to perform an act that is unreasonable, an act in contradiction with his total will, his total personality, the following may happen: In spite of his profound hypnosis, the hypnotized man may refuse to obey . . . or awake from his hypnosis if the demand of the hypnotizer as well as the total situation are no longer in accord with his other ego ideals." Hollander (1, 40) says, "Even a person in the hypnotic state will refuse to perform any act which is contrary to his or her natural disposition. But both normal and hypnotized people will readily accept any notion for which their own nature has already prepared them." Loewenfeld said in 1922 (2, Ch. 14) that to date "there is not a single well-authenticated case of severe crime perpetrated through hypnosis."

The subjects were hypnotized in a room across the hall from the large room in which the experiment proper took place.

Apparatus. A large box, open on the front side, was constructed; in the interior was placed a large sheet of glass, bent in such a way that all reflections were cast upward against a black baffleboard roof and there absorbed. In front of this sheet of glass was placed hardware cloth cut in the center and turned back in such a way that the whole box gave the appearance of being an animal cage. The glass in back of the hardware cloth was totally invisible.[2] The experimenter tested this fact by having several of his colleagues come in and reach for a hammer that was placed some distance back of the glass. Not one of them saw the glass, and one woman professor injured her finger when she struck the glass as she reached for the hammer.

A large diamond-back rattlesnake[3] was placed in the box. The animal was allowed to stay in a warm room in the box three days until it became very lively. To add to the activity of the snake during the experiment, holes were bored in the box in many places and the snake poked by means of a wire to incite him to action. The result was a most frightful scene. The snake invariably wound himself into a coil with his head lifted ready to strike, his rattles singing loudly enough that they could be heard within a radius of 100 feet.

Condition I. (Subjects A and B.) The subject was placed in the room with the stimulus box and thirty feet from it. The following instructions were given:

"Do you see that box?" When the subject said, "Yes," the experimenter went on to say, "There is a piece of coiled rubber rope in that box. Go up to the box, reach through the screen wire, and pick it up."

Subject A went up to the box slowly, without perceptible change of pace until she got to the box. She seemed surprised at first touching the glass. She hesitated a moment and then began to explore the surface of the glass for an opening. Finding no opening, she spread her hand on the glass and began to push. Even though the glass was of heavy plate, some danger was involved if she pushed too hard, and the experimenter asked her to stop exploring the glass.

Asked one week later, while awake, if she could conjecture why she had been willing to expose herself to such a great danger, subject A said that she did not know why, but that she supposed she was confident that the experimenter would not allow her to be harmed. Actually, subject A remembered nothing about the incident, and was only giving her judgment as to the cause of her behavior.

Subject B went immediately to the box, saw the snake, turned around facing the experimenter and awoke. The experimenter asked if he were awake and he said that he was. (It is only fair to say that the experimenter was sceptical beforehand of the depth of trance of this particular subject.)

Condition II. (Subjects C and D.) The hypnotized subjects were taken into the experimental room in full sight of the cage (or box), but thirty feet from it, and asked these questions; the answer, "Yes," was expected and received in every case:

[2] The apparatus is being manufactured by the C. H. Stoelting Co., who issue a leaflet describing it. The device is called the "Rowland Stimulus Box."

[3] Furnished and handled by Mr. Hugh Davis, Curator of Mohawk Zoo, Tulsa, Oklahoma.

"Have you ever seen a rattlesnake? Is a rattlesnake dangerous? Do you see that box? There is a rattlesnake in that box. Go up to it and reach through the opening in the screen wire and pick up the snake. Go right ahead."

Subject C went directly to the box, stopped briefly when she came in sight of the coiled snake, but when the experimenter [4] urged her by saying, "Go ahead," she thrust her hand through the wire opening and gave evidence of surprise when she touched the glass. She explored the surface area of the glass but showed no tendency to force an opening through the glass as A had done.

Subject D went directly to the apparatus, reached in and touched the glass. She touched the glass in several places until, finding no opening, she stopped and took two steps back from the box and stood, apparently uncertain as to what next to do.

PART II

In this part of the experiment the object was to see whether subjects would perform acts unreasonably dangerous to others.

The apparatus was modified by removing the snake, and the hardware cloth from in front of the glass. Further, a section 12 x 14 inches was cut from the back of the box, and against this opening the head of the author appeared during the part of the experiment about to be described. The author's face was well lighted and he read from a book. It must be remembered that the subjects saw nothing intervening between themselves and the author.

The subjects participating in Part II were as follows:

Subject E, female, 16 years of age, a high school student.

Subject F, male, coach of high school athletics, a graduate student.

The procedure was as follows: The subject, hypnotized, was brought into the presence of the stimulus box. Thereupon the experimenter poured sulphuric acid from a bottle into a glass, and, in the presence of the subject, dipped a strip of zinc into the acid. The reaction was strong, with fumes steaming from the mouth of the glass. The subject was then asked, "Do you know what sulphuric acid is?" and regardless of the answer the explanation was given that "sulphuric acid is very dangerous. It will scar the skin and put out the eyes." The subject was then told to pick up the glass and throw the acid at the author's face, the author being protected, of course, by the invisible glass.

Subject E picked up the glass and was very reluctant to throw the acid. She hesitated; the experimenter insisted and finally said, "I'm going to count three and when I finish you will throw the acid. One, two, three." The subject threw the acid, set down the glass, shuddered and covered her face with her hands. She seemed very much disturbed.

Subject F was instructed in the same way as Subject E. He picked up the acid, hesitated a moment, and then threw it with such force that the acid spread all over the surface of the invisible glass.

By way of a control, forty-two persons, of every age and degree of sophistication, were asked to come to the laboratory and pick up the snake, as in Part I of the experiment. With one exception all the persons were not only badly frightened at the appearance of the snake, but would not come close to the box; only a few were persuaded finally to pick up a yard stick and

[4] Dean L. S. McLeod, the author's colleague in psychology, was experimenter for Condition II of Part I, and Part II.

try to touch the snake. They all seemed bewildered when they touched the glass which they could not see. The exception referred to was as follows: A young woman was told to reach for the snake and she did so at once, of course striking the glass. When asked why she complied so readily, she said that of course it was an artificial snake and she was not afraid of it. Assured that the snake was real, she made a closer examination of it. She then became frightened, and, even though she now knew she was protected, would not go near the box.

CONCLUSIONS

Within the limits of this experiment it seems possible to conclude that:

1. Persons in deep hypnosis will allow themselves to be exposed to unreasonably dangerous situations.

2. Persons in deep hypnosis will perform acts unreasonably dangerous to others.

A possible explanation, hinted at in two places in the account, is that confidence in the hypnotist causes the subject to forego his better judgment.

If the above conclusions be true, it follows as a very practical application that only professional psychologists and others adequately prepared should be permitted to make use of deep hypnosis.

The author feels that the common acceptation that hypnotized persons will not perform acts that violate their ideals is badly in need of re-examination.

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A STUDY OF CONFORMITY TO A TRAFFIC REGULATION

BY NORMAN FREDERIKSEN, GEORGE FRANK, AND HERBERT FREEMAN
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F. H. ALLPORT has pointed out the need for studying not only modes of agreement with respect to social behavior, but also the variability exhibited by people in satisfying social requirements.[1] It is not enough to know the accepted attitude, custom, or folkway; we must also study the frequency of performance of acts representing various degrees of conformity to the attitude, custom, or folkway.

Such a study can be made by using a *telic continuum,* the steps on which represent various degrees of satisfaction of the purpose of a law or convention. For example, *stopping completely* at an intersection where there is a red traffic light is an act representing complete fulfillment of the purpose of the traffic law governing that situation. *Slowing down considerably,* but not stopping, is the next recognizable degree of fulfillment of the purpose of the law. *Slowing down slightly* is the next step on the telic continuum, and *going ahead without reducing speed* is an act representing no fulfillment of the purpose of the traffic regulation. Allport found that when the frequency of occurrence of these acts was plotted against the steps on the telic continuum, the mode of the resulting distribution curve was located at that extreme of the continuum which represents complete conformity; the curve dropped rapidly from this mode and then fell more slowly as the opposite end of the continuum (representing complete non-conformity) was approached. The curve roughly resembled a reversed letter *J* in shape, and was accordingly called a *J-curve.* Allport reported the results of twelve other studies of conforming behavior in political, economic, and religious situations in which telic continua were used. Curves of similar shape were usually obtained. He suggested, as a result of these studies, the following hypothesis: "In a field of conforming behavior [i.e., one represented by a distribution curve where at least 50 per cent of the cases fall on the step corresponding to complete conformity] degrees of conformity are distributed upon their appropriate telic continuum in decreasingly diminishing proportions as one proceeds from the most widely practiced form of the act toward the greatest recognizable deviation from that form." [2]

The shape of the J-curve is thought to result from the cooperation of at least four sets of factors. *Conformity-producing agencies,* such as coercion, propaganda, and use of institutional symbols, tend to heighten the mode at the point of complete conformity. *Common biological tendencies,* such as the need to satisfy hunger and thirst and the desire to resist thwarting of one's activities, tend to produce a mode which may or may not coincide in position

[1] F. H. Allport. The J-curve hypothesis of conforming behavior. *J. soc. Psychol.,* 1934, **5,** 141–183.
[2] *Ibid.,* 161.

with the one produced by conformity-producing agencies, depending upon the particular set of circumstances. *Personality-trait differences,* such as those related to ascendance-submission or promptness-dilatoriness, tend to produce a spread in the distribution according to the law of normal distribution. *Chance,* the last of the four factors, would tend to produce a perfectly flat curve, since by chance alone the acts on the telic continuum might be expected to occur an equal number of times. The shape of the distribution on a telic continuum for any particular institutional situation is thought to be a combination of the four curves which would result from the four sets of factors considered separately.

PROCEDURE

In the present investigation, observations were made of the degrees of conformity exhibited by motorists in a type of traffic situation not previously studied. The conformity exhibited by 12,666 automobile drivers to a traffic regulation requiring motorists to keep in line at a sharp right angle turn in a New York City highway was observed. This turn is located in what is, according to the Department of Plants and Structures of the City of New York, the most travelled stretch of road in the city. Two lanes in the highway lead in an uptown and two lanes lead in a downtown direction, the lanes being separated by white lines painted on the pavement. Large prominently-displayed signs direct the motorists to "KEEP IN LINE." One observer recorded the behaviors of the uptown motorists, another the behaviors of the downtown drivers. Because of the speed of the traffic, the narrowness of the lanes, and the sharpness of the turn, considerable effort is required on the part of the drivers in this situation to comply fully with the requirement by staying entirely in the proper lane.

The steps on any telic continuum represent degrees of fulfillment of the purpose of the prescribed act. In this situation, the prescribed act is to keep entirely in the proper lane, and the purpose of the act is to secure the maximum amount of safety of those driving in traffic. Four easily distinguishable acts represent successive degrees of satisfaction of this purpose. *Staying in line,* the first step in the telic continuum, represents the maximum degree of fulfillment of the purpose of the regulation. *Crossing the white line less than half a car's width* is the next distinguishable degree of satisfaction of the purpose of the law. *Crossing the white line more than half a car's width,* but not cutting lanes, is the third step in the continuum, representing still less satisfaction of the purpose. Finally, *cutting lanes,* or going entirely from one lane to another, represents no fulfillment whatsoever of the purpose of the law. A number of collisions were witnessed by the observers, all of them caused by cutting lanes.

As each motorist passed the observer, a tally was made on a record blank to indicate on which of the four steps on the continuum the behavior of the driver fell. Separate symbols were used to indicate three classes of drivers: chauffeurs, taxi drivers, and drivers of private automobiles. A traffic policeman went on and off duty several times during the observations; records were kept separately for the times when he was and was not present at the turn. The observations were made on a single day from 9:00 A.M. to 11:00 A.M., from 11:30 A.M. to 1:00 P.M., from 2:00 P.M. to 3:00 P.M., and from 4:30 P.M. to 5:30 P.M.

FIG. I

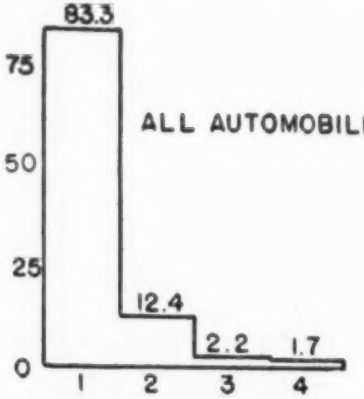


FIG. II

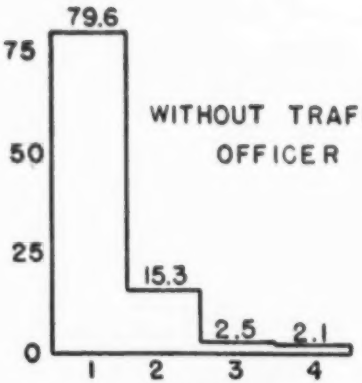


FIG. III

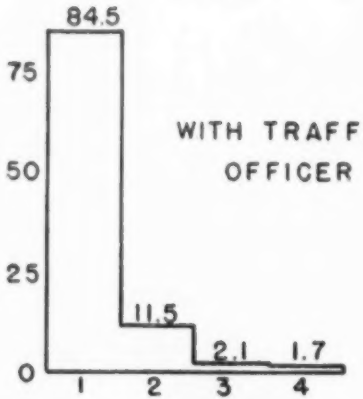


FIG. IV

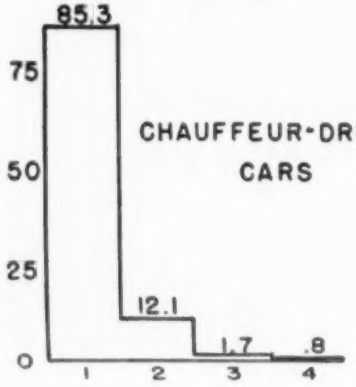


FIG. V

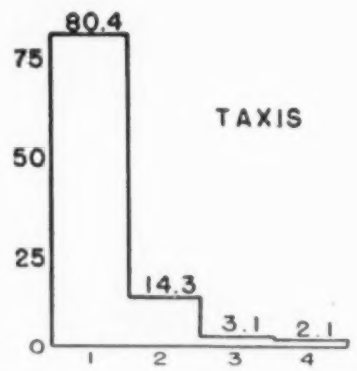
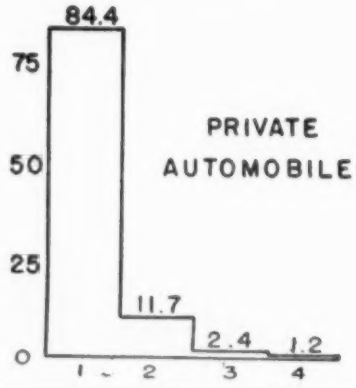


FIG. VI



RESULTS

Figure 1 is the distribution curve of all the motorists on the continuum we have described. A total of 12,666 automobiles was observed. Of these, 83.3 per cent fell on step 1, which represents complete conformity; 12.4 per cent fell on step 2, representing those who crossed the white line less than half the width of a car; 2.2 per cent fell on step 3, representing those who crossed the white line more than half the width of the car; 1.7 per cent exhibited complete non-conformity by cutting lanes (step 4). The distribution curve, steepest at the position representing complete conformity, diminishes steadily and at a diminishing rate to the position representing non-conformity. The curve is a typical J-curve, which tends to verify Allport's hypothesis that in a field of conforming behavior, the distribution of degrees of conformity upon the proper telic continuum is in the form of a J-curve.

The traffic officer stationed at the scene of the observations was off duty for a part of each hour during which observations were made, with the single exception of the period from 4:30 P.M. to 5:30 P.M., when he was on duty the full hour. Figure 2 is the distribution curve representing the behavior of the motorists who were observed during the time when he was not on duty. 79.6 per cent of the 3,079 automobiles observed under this condition showed complete conformity, 15.3 per cent fell on step 2, 2.5 per cent fell on step 3, and 2.1 per cent showed complete non-conformity. Figure 3 represents the distribution curve for those who were observed during the time when the traffic officer, in uniform and in plain view of the drivers, was on duty at the turn. Under this condition 9,587 automobiles were observed. 84.5 per cent of them conformed completely, 11.5 per cent fell on step 2, 2.1 per cent fell on step 3, while 1.7 per cent showed complete non-conformity.

Both of these curves are J-curves, but Fig. 3, representing motorists' behavior in the presence of a traffic officer, is steeper and has a higher mode than the other. The difference between the percentage of cases on the mode in Fig. 2 and the percentage of cases on the mode in Fig. 3 is 4.9 per cent. The SE of that difference is .00817, and the critical ratio of the difference to its SE is 6.0. There is, then, a significant difference between the proportion of motorists who show complete conformity to the regulation in the absence of a traffic officer and the proportion who show complete conformity in the presence of an officer. A comparison of the two curves will show that the principal influence of the traffic officer is to shift cases from step 2 to step 1; in other words, his presence influences motorists who would otherwise cross the white line slightly to stay entirely in the proper lane. The differences between the two curves seem to be clearly attributable to one of the conformity-producing agencies, namely, the traffic officer, since all other factors were apparently constant.

Fig. 4 presents the distribution curve based on observation of chauffeur-driven automobiles. Those cases where the driver was clearly identified as a chauffeur by his uniform or cap were included in this category. Of the 1,308 chauffeur-driven automobiles, 85.3 per cent showed complete conformity, 12.1 per cent fell on step 2, 1.7 per cent fell on step 3, and only 0.8 per cent showed complete non-conformity. Fig. 5 shows the distribution curve for the taxis; 3,632 such automobiles were observed. 80.4 per cent showed complete conformity, 14.3 per cent fell on step 2, 3.1 per cent fell on step 3, and 2.1 per cent showed complete non-conformity. Fig. 6 is the curve for private auto-

mobiles, most of which were presumably driven by their owners. Of the 7,726 such automobiles observed, 84.4 per cent conformed completely, 11.7 per cent fell on step 2, 2.4 per cent fell on step 3, and 1.2 per cent showed complete nonconformity.

All three of these curves are J-curves. The curve for the chauffeurs indicates the greatest conformity and the least nonconformity. The difference between the per cent of their cases falling on the mode, 85.3, and the per cent of taxis falling on the mode, 80.4, is 4.9 per cent. The SE of this difference is .01180, and the critical ratio of the difference to its SE is 4.2, indicating a reliable difference between the proportion of chauffeurs and the proportion of taxi drivers who conform completely to the regulation governing the situation.

The proportion of drivers of private automobiles who conform completely is slightly lower than the proportion of chauffeurs; 84.4 per cent conformed completely. The difference in the percentage of cases on the mode between drivers of private automobiles and chauffeurs is 0.9. The SE of the difference is .0106, and the critical ratio 0.9, indicating no reliable difference.

The difference in the percentage of cases on the mode between drivers of private automobiles and taxis is greater, 4.0 per cent. The SE of this difference is .0078, and the critical ratio 5.2. There is, then, a statistically reliable tendency for drivers of private automobiles to conform completely more frequently than taxi drivers.

The high amount of conformity and low amount of nonconformity exhibited by chauffeurs is probably attributable to a strong conformity-producing agent in the person of the employer. Drivers of private automobiles showed nearly as much complete conformity, but at the same time considerably more complete nonconformity than chauffeurs. The fact that the personality characteristics of the drivers of private automobiles are expressed more completely probably accounts for this difference. The right-hand part of the distribution curve for private automobiles would probably coincide more nearly with a personality trait distribution than would the curve for chauffeurs. Traits such as recklessness or aggressiveness would not be allowed full expression in the chauffeurs. Taxi drivers, on the other hand, are motivated by the need to discharge passengers as quickly as possible in order to get new fares. Their cabs are usually owned by corporations and are heavily insured. Drivers are, therefore, less concerned about inflicting minor damages to their vehicles.

Evidence regarding the relationship between time of day and shape of the distribution curves is not clear cut and is therefore not presented in detail. There is some evidence, however, that during those hours when there is heavy downtown traffic bound for the commercial area of the city, the amount of conformity is reduced; and during the hours when more leisurely drivers, such as shoppers and tourists and homeward-bound drivers from the commercial area, are on the road, there is a greater amount of conformity to the regulation. These differences might be attributable to Allport's factor of biological tendency.

SUMMARY

1. The frequency of occurrence of acts representing degrees of satisfaction of the purpose of a traffic regulation requiring motorists to keep in line were plotted against the telic continuum. The curve representing the behavior of the 12,666 motorists observed in the study was a steep J-curve with 83.3 per cent of the cases on the mode.

2. Four and nine-tenths per cent more motorists showed complete conformity to the regulation when a traffic officer was present than when no officer was present. This difference is statistically reliable. The effect of the presence of the traffic officer was primarily to influence motorists who would otherwise cross the white line slightly to keep in line completely.

3. Chauffeurs exhibited most conformity and least nonconformity, taxi drivers least conformity and most nonconformity, while drivers of private automobiles were intermediate in both respects. Four and nine-tenths per cent more chauffeurs than taxi drivers conformed completely; the difference was found to be statistically reliable. Four per cent more drivers of private automobiles than taxi drivers conformed completely. This difference is also statistically reliable. There was no reliable difference between chauffeurs and drivers of private automobiles with respect to the proportion showing complete conformity.

SOME PSYCHOLOGICAL FACTORS IN ACCIDENT-REPEATER DRIVERS

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AND
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A DRIVER clinic [1] was operated in the Bureau of Motor Vehicles, New York City, for several months, through the courtesy of Commissioner Charles A. Harnett. A notice was handed to accident-drivers at the end of their hearings inviting them to take the test. A surprisingly large number came in on this voluntary basis.

A total of 142 repeaters and 52 volunteers were tested. The number passed through the clinic per day varied from three to eighteen. On account of the reduction of the size of the hearing calendar during the summer vacation, the number of persons available for testing was small.

Numerous drivers taking the tests were quite profuse with their thanks and later sent friends to the clinic. Often attorney, client, and friends who attended the hearing as witnesses, came in with the driver and ended by taking the tests. In a few cases, there was some suspicion as to the purpose of the clinic, but this doubt was quickly allayed when it was explained that the testing was being done for the education of the driver himself and for research purposes.

ANALYSIS OF RESULTS

The accident repeaters as a group were compared with the volunteer drivers as a group in order to show relations between average test scores and accidents. The accident repeater group was poorer on the average than the volunteer group on the following tests:

1. Accelerator Reaction Time. The time from the appearance of a light signal until the subject lifted his foot.

2. Braking Reaction Time. The time from the appearance of a light signal until the subject lifted his foot from the accelerator and moved it over and applied the brake.

3. Vigilance Braking Reaction Time. The operation described in (2) above performed while the subject was taking the steering test (4).

4. Simple Steering. The proportion of a minute that a subject was able by means of a steering wheel to keep the hood of a miniature car lined up with a moving road scene.

5. Vigilance Steering. The operation described in (4) performed while the subject was responding to the braking reaction signal (2).

6. Vigilance Balance. The ratio between scores on tests (2) and (4) and those on tests (3) and (5).

[1] See *J. appl. Psychol.*, 1938, **22**, 59–69; or *Tech. Rev.*, 1938, **40**, 309–312; 312; 328 for descriptions of the clinical apparatus.

7. Speed Estimation. Two objects moving at right angles to the subject's line of vision appear for an interval and then disappear behind a screen. The subject estimates the point on the screen where the faster object will catch up with the slower moving one, and the error is noted.

8. Miniature Highway Test. The number of safe passes is indicated on a complex apparatus which attempts to measure estimation of speeds and manipulation of motor vehicle controls.

9. Glare Vision. The amount of light necessary for a subject to see a test object while a glaring light shines into his eyes, gives an indication of the subject's sensitivity to glare.

TABLE 1

AVERAGE TEST SCORES: VOLUNTEER AND REPEATER GROUPS
NEW YORK CITY
APRIL–JULY

	AVERAGE SCORES *	
	Volunteers	Repeaters
Accelerator Reaction Time	.187 sec.	.203 sec.
Braking Reaction Time	.422 sec.	.455 sec.
Simple Steering	52.8 per cent	47.7 per cent
Vigilance Steering	42.4 per cent	39.1 per cent
Vigilance Braking Reaction Time	.626 sec.	.668 sec.
Vigilance Balance	—18 units	—27.6 units
Speed Estimation	2.2 units	2.8 units
Miniature Highway	9.8 safe passes	4.1 safe passes
Glare without Glasses	3.1 units	2.8 units
Glare with Glasses	0.8 units	0.4 units

* For all tests except reaction time, vigilance balance and speed estimation, high score=good ability. The number of volunteers was 52 and that of repeaters 142 in all tests except number 8, where the volunteers numbered 15 and the repeaters 62.

Table 1 shows that the deficit of the repeater group ranged from 6 per cent to 50 per cent for the various test scores. The general results reported here have been corroborated in other cities where a clinic has operated.[2]

VISUAL TESTS

A relatively small percentage of drivers failed the visual tests (see Table 2). In those cases where visual defects were present they were often found to be obviously related to the accidents. In peripheral vision, 2.7 per cent of the

[2] See *Driver clinic results*, a free monograph procurable from the senior author.

accident repeaters showed a reduction of side vision amounting to 30° or more. In the majority of cases this reduction occurred in both eyes although a number showed it in one eye only. Both groups of drivers showed from 5 per cent to 10 per cent with questionable hearing (a reduction of 20 decibels or more). The percentage showing excessively poor hearing was negligible.

There were few repeaters who failed the test for visual acuity. This fact might be taken to indicate that by and large the eye test given by the motor vehicle department has been successful in weeding out cases of poor vision, or at least impressing those with poor vision with the need for correcting it with glasses or by other means.

TABLE 2

Visual Tests: Volunteers and Repeaters
New York City
April–July

	Per Cent of Drivers Reporting Characteristic	
	Volunteers	Repeaters
Failed Peripheral Vision (120° or less)	0.	2.7
Failed Acuity Both Eyes	1.9	0.7
Failed Depth Perception	11.4	12.4
Failed Astigmatism	3.8	0.7
Hearing Questionable (20 to 30 db loss)	5.7	9.7
Hearing Poor (40 or more db loss)	3.8	0.7

Personal Characteristics

A questionnaire, included on the test score blank and filled out by the driver before taking the tests, elicited certain information. Table 3 summarizes the results from the questionnaire returns. In interpreting these results, it should be borne in mind that the number of drivers included is a comparatively small sample and the results are therefore tentative.

The foreign-born appeared to a much larger extent among the accident-repeater group than among the volunteers (32 per cent as against 2 per cent); the largest single group were those reporting Russian birth (10 per cent). Since the 1930 census indicates that 34 per cent of New York's population is foreign-born, however, it would seem that the foreign-born drivers furnished only a normal share of accident repeaters.

A slightly larger percentage of the repeaters reported that they were taught to drive, another factor which varies from city to city. The excess percentage is not large enough to be very significant, but the figures do indicate that approximately one-half of the drivers of both groups learn to drive by themselves.

Both groups reported driving for the most part during the day. The repeaters reported a higher proportion of city driving.

An unusual relationship is shown with respect to the type of car driven by the two groups. The repeaters showed an exceptionally large percentage driving medium and heavy weight cars probably of older vintage.

TABLE 3

DIFFERENCES IN PERSONAL CHARACTERISTICS: VOLUNTEERS AND REPEATERS
NEW YORK CITY
APRIL–JULY

	PER CENT OF DRIVERS REPORTING CHARACTERISTICS	
	Volunteers	Repeaters
American born	98.1	68.9
Russian born	1.9	10.3
Father:		
American born	48.0	22.8
Russian born	15.3	24.8
Mother:		
American born	48.0	25.5
Russian born	15.3	23.4
Were taught to drive	42.2	50.3
Married	36.4	60.0
Divorced	0.0	2.7
Occupation:		
Professional	46.1	2.7
Clerical	17.3	4.1
Skilled	9.6	13.8
Semi-skilled	5.7	51.0
Drive mostly:		
During night	13.3	18.6
During day	47.9	48.9
In city	59.4	77.2
Both city and rural	28.7	14.4
Drive light car	30.7	16.5
Drive medium car	36.5	42.8
Drive heavy car	15.3	35.2
Wear glasses:		
To drive	13.4	4.8
Both read and drive	21.1	13.1
No glasses	48.0	68.9
Type of car driven:		
Private	78.8	48.9
Truck	1.9	12.4
Taxi	3.8	17.9
Private and commercial	3.8	9.7
Grip:		
Tense	5.7	16.5
Finger	0.0	5.5
Wheel manipulation:		
Jerky	17.3	32.4
Number of children	0.6	1.09
Driving experience	9.3 years	11.8 years
Years of education	14.1 years	9.1 years
Yearly mileage	11,300 miles	14,000 miles
Preferred open road speed	45.4	37.4

The repeaters included a greater number of truck and taxi drivers (who usually have a greater yearly mileage) than the volunteers. This finding is in line with the results from other cities, but the percentage of commercial drivers is higher than usual in New York City.

The repeaters were found to operate in a tense and jerky manner much more frequently than the volunteers and to include a greater number of operators who handle the steering wheel with the tips of the fingers. These characteristics probably reflect inferior motor ability and corroborate the test results in Table 1.

The repeaters have more children than the volunteers, a characteristic which goes with lower occupational and economic status.

Driving Experience—Yearly Mileage—Education

The accident repeaters show approximately 20 per cent longer driving experience than the volunteers. They also report a greater yearly mileage. The volunteer group, however, reports a yearly mileage which is lower than that usually found for the general driving population (as might be expected on account of the congestion in New York City), and is therefore probably not representative in this particular. The repeater group shows an average schooling of about 9 years, as against 14 for the volunteers.

The repeaters report a preferred open road speed of about 8 miles per hour less than the volunteers. This preference checks with findings in other localities, but the reported speed of the accident repeaters in all cities is probably fictitiously low because of an attempt on their part to appear conservative and law-abiding.

Summary

The present report gives the results of an analysis of the data obtained from the operation of a Driver Research Clinic in the New York Motor Vehicle Department. It was found that *the accident repeaters were poorer as a group in the various tests.* This deficit as compared with the volunteer group ranged from 6 per cent to 50 per cent of the average test scores.

Contrary to findings elsewhere, the proportion of foreign-born among the repeaters was about that to be expected from the make-up of the total population as shown by the census. The repeaters reported greater driving experience and yearly mileage but about 4 years less education. The volunteer group which was obtained was higher in economic status and education, and slightly lower than average in yearly mileage.

The attitude of the repeater drivers taking the tests was very favorable. On this account it is felt that a marked educational effect will show itself in their subsequent accident history. Such an effect has been noted by means of follow-up studies that have been made of accident repeaters who have attended a clinic elsewhere.[3]

The results of this investigation corroborate findings from other similar studies. They point to the need of a diagnosis and treatment of violators and accident drivers. Bad drivers are not born that way. They drive dangerously because of lack of training, or as a result of imitating other bad drivers, or because they possess sensory or motor or temperamental defects of which they are ignorant. The solution lies in the clinical re-examination of all violators and accident repeaters.

[3] See *Safety Engineering*, 1938, **75**, 13.

ON THE ENIGMATIC PERSONALITY OF AMIEL

BY E. L. TALBERT
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THE *Journal intime* of Henri-Fréderic Amiel reflects the struggles of an inhibited Protestant of Geneva whose inner experience was in sharp contrast with the routine of a university professor. Like Montaigne, he made no pretension to system and universality; unlike him, he possessed no sparkling humor which permitted him to enjoy the foibles of mankind, including his own. Montaigne, living in the divergent currents of an age of secular curiosity, searched for individual differences in all levels of being, and accepted with appreciation, if not with praise, the sort of creature which Mother Nature had made of him.

Amiel was unable to feel at home with the picture of himself which his diagnosis furnished. Obsessed by the absolutes and perfections which Calvinism had instilled into his childish imagination, he regarded his career as one of failure. His reading of metaphysics and Oriental speculation strengthened his bent for *devaluation* of the individual instance. The attempt to see the world of phenomena from a misty tower made him thin and unconvincing when he turned his attention to the "world"—of science, invention, and social movements.

The secret of Amiel remains pretty largely a secret, after fifty years of discussion. Did the author of the *Journal* plan to baffle the reader by the very volume of the revelation? Was it his intent to render a clear pattern of his self impossible to trace?

Since the publication of excerpts from the seventeen thousand pages of his manuscript, critics have examined his thought from the angle of their special interests. Their pronouncements upon the complex personality of Amiel have become a fixed frame of reference for subsequent readers, difficult to change because of the halo of authority. The English-reading public derived a first impression from the translation in 1883 of selections from the *Journal* by Mrs. Humphrey Ward, and especially from her evaluation written in the *Introduction*. Mrs. Ward seizes upon the *high-minded* Amiel, knight of the spiritual life, who finds faith beyond science and theology. It will be recalled that Mrs. Ward was one of the Victorians who were troubled by Strauss, Comte, Darwin, and Mill.

The French-reading public has the collections by Scherer and Bouvier. These too select the portions exhibiting Amiel the contemplative, the seeker after the ineffable, the introspectionist, the lover of beauty, and the critic of books and men.

Another school of thought seeks to prove that Amiel is a pathological case. He is a victim of aboulia—fearful, incapable of decision, recessive; in time, so the theory goes, the condition develops to the point where Amiel is unable to distinguish reality from phantasy. He finds satisfaction in objectless knowing and feeling, in passing beyond the precincts of individualized mental

process. This viewpoint stresses that phase of Amiel which he calls the "Brahamanizing" tendency.[1] Bourget's elaborate study analyzes this facet of Amiel.[2]

The arrangement of *Journal* entries published under the title *Philine*[3] and translated by Van Wyck Brooks is more revealing of Amiel as a human being than that contained in Mrs. Humphrey Ward's translation. Previous excerpts from the mass of the *Journal* had omitted reference to the love affair of Amiel with a young widow whom he called "Philine." M. Bernard Bouvier, M. Charles du Bos, and M. Jacques Schiffrin bring together the accounts of meetings of Amiel and his mistress, his comments upon her conversation and letters (her words of love he calls "litanies"), and the soliloquies following their contact. The story of the liaison furnishes a unifying base upon which to exhibit Amiel's thought and conduct when stimulated by the deepest sexual attachment he is known to have experienced. The excerpts indicate that for a long stretch of years Philine, an accomplished and attractive woman, gave Amiel all a woman can offer, that she praised him extravagantly, that he recorded her laudations meticulously, that after long meditations which canvassed the charms and demerits of Philine, he decided in 1878, three years before his death, that "a marriage is not advisable unless it is irresistible."

The passages leading up to this choice are in the familiar vacillating vein of the "modern Hamlet." He admits the admirable qualities of Philine and her worth to a lonely man, and at times resolves upon marriage. Then the inhibitions appear. He has scruples against imposing his wishes upon another's personality. An ascetic withdrawal from bodily contact is revealed. Constant intimacy of any kind he wishes to avoid. He fears the disturbance to his settled mode of life, as he also fears what relatives and people in general would say, should he marry. Growing suspicious and censorious of Philine, he accuses her of incapacity to answer to his needs. Numerous passages describe the marital unhappiness of the families of his acquaintances. Psychologists have names to cover these pronounced symptoms.

The close and coherent picture of Amiel derived from the *Philine* collection warrants not an abandonment but a revision of three accepted generalizations. One of them is the over-simplified version of Amiel as a dreamy, impractical person with a pathological inability to make up his mind about anything. *Philine* reenforces the then heretical judgment expressed in 1923 by Dr. Joseph Collins:

> He discharged the duties of a professor from the time he was twenty-eight until he was sixty. He poetised pleasantly; he communed with nature and got much pleasure from it; and he had very definite social adaptability. His general level of behavior was high. He was a diligent, methodical worker; he reacted in a normal way to conventional standards; he had few personal biases or peculiarities and none that drew particular attention to him; and he seemed to have adjusted himself without great difficulty to the incidences of life that he encountered.
>
> To say that such a man was the victim of some obscure psychosis is either to speak beyond the facts or to speak from the possession of some knowledge that is denied one familiar with his writings and what has been written about him.[4]

It is noteworthy that none of his contemporaries thought him abnormal, at

[1] Amiel, F. *Journal* (translated by Ward). New York: Macmillan, 1929, Vol. 2, 189–191.

[2] Bourget, P. *Essais de psychologie contemporaine.* Paris: Plon-Nourrit, 1883, 298–305.

[3] *Philine.* Boston: Houghton Mifflin Company, 1930.

[4] Collins, J. *The doctor looks at literature.* New York: Doran, 1923, 225.

least not beyond the idiosyncrasies expected of a college professor. Amiel had men friends, he had women friends to whom he acted as confessor and guardian, he played with children, who liked him, he visited relatives, and enjoyed picnics. The external Amiel, then, is moderately well-balanced; it is the subjective Amiel who is seldom of one mind; and his years of study in Germany and his reading of Hegel fed his bent for self-analysis.

A second stereotype needs correction. Passages in *Philine* give us pause before we accept the dogma that Amiel joined the company of the mystics. True, he asks for quiet and isolation from over-stimulation. Like other cognitive introverts, he delights in the play of ideas for their own satisfying value. The inability of the intellect to reach a thoroughly secure stopping-point he keenly realizes. In the Whole is truth, and reason cannot find it: individual personality is an abstraction, he declares. There are passages in the *Journal* which, on their face, appear to ratify the philosopher's sentence, "All determination is negation." His later years of illness added a physiological reason for his withdrawal from practical demands.

To offset the impression that the "Hindoo" in him was a major trait, there is a passage in *Philine:*

> This combustion of the ego, this abandon of the individuality, this total forgetfulness of self, I have never felt in its full sweetness except through poetic admiration or through moral or intellectual emotion, but even then, never, I think, to the depths of my being, to my very marrow. Neither ecstasy, nor rapture, nor intoxication, nor amazement have yet snatched me from the consciousness of my self, have conquered, overwhelmed and absorbed me.[5]

In his preface, Edmond Jaloux calls attention to the sophisticated preparation for the onset of "Brahmanizing" states. So much self-conscious technique is most consistent with the conclusion that in this field, too, there is the same core of ego-centrality which marks his relation with Philine.

A third commonly accepted version may be questioned in the light of the narrative available in the *Philine* volume. It is said, following Amiel's own assertion, that Amiel had no will, and that if he could have summoned all his potentialities he would have produced that high-grade work, academic and literary, whose absence he bewails. With the material of *Philine* before him, Jaloux writes:

> He believed all his life that he was not energetic enough to exert his will, but in truth he never exerted his will because there was nothing he wished especially to do or say. What would an energetic Amiel have become? Evidently a very mediocre writer, possibly a critic, but neither a poet, philosopher, nor novelist. All this he knew intuitively if not definitely. He suffered from never doing anything great, for he wished to be great at any price, and he became so in a way he would never have dreamed of expecting. He attributed, then, all his miseries to what was really a very wise and instinctive prudence which forbade him to undertake things for which he was not fitted, and demanded, on the contrary, that he reserve his forces for an enterprize almost unique in the world, and which has given him the most enviable immortality.[6]

It is probable, moreover, that a consciousness of the modest range of his abilities and of his ego-centrality played a part in his rejection of marriage. Close intimacy is not always the setting for hero worship. He returns with inner compulsion to the compensatory composition of the *Journal*. Concluding his essay, Jaloux acutely remarks:

[5] *Philine*, 79.
[6] *Philine*, Introduction, x–xi.

He needed that inner torment which gave birth to his nobility and without which he would have been a mediocre and timid small townsman; he needed to obtain from himself that cosmic contemplation which was his true form of happiness and of which the colorless routine of a narrow home would no doubt have deprived him; he needed above all to preserve in his spirit those contradictory and diffuse states in which his passionate desire for the inner truth found such fields of expression and observation. The real Amiel was no longer Amiel himself, it was in his *Journal*,—everything for him was subordinate to this immense enterprize. Prodigious example of a man who cut himself off from life, in order to record at his ease the image of that life! [7]

Returning to Amiel the lover, the reader is struck by the difference between the Amiel of the previous editions and the Amiel who walks with Philine, to meditate afterward upon her traits and the probable consequences of marriage. A measure of isolation from the woman who arouses his body and emotion is disclosed. There is struggle ending in failure to sympathize with her, to assume her role, in the phrase of Professor G. H. Mead, and to modify his attitudes in terms of an imaginative project of the self of Philine. The personal relation with women which best suits him appears to be the superior status of confessor and adviser, a status in which segmental reactions are enough. Communication with Philine grounded upon equality is irksome.

In the Ward translation, on the other hand, he lives the life of the universe; he identifies himself with the achievements of men through the cool medium of symbols. Seemingly it is easier for him to feel at one with abstract humanity than with Philine. The strong turn for internal debate and aloofness supplies perspective and wisdom to his cogitations on the theoretical problems of human destiny. The same qualities unfit him for response as a total self to a demanding near-at-hand woman.

[7] *Ibid.*, xxxviii–xxxvix.

REVIEWS

EDITED BY EDNA HEIDBREDER

SOCIAL PSYCHOLOGY. By Daniel Katz and Richard L. Schanck. New York: John Wiley and Sons, 1938. Pp. xiv+700.

Most textbooks bearing the ill-defined title "Social Psychology" have emphasized either psychological sociology or sociological psychology. The present work avoids these two positions by concentrating on human beings doing things to each other and together acting upon objects. Language and concepts are used from both sociology and psychology, depending upon the phenomenon under discussion. It has become obvious to many scientists curious about the behavior of people that the classical division into psychology, sociology, economics, political science, etc., is rather fruitless. Probably the labels have value in very roughly indicating to the student the general types of problems dealt with. If further employed as communication, however, the labels are worse than useless. They often imply that life is split into cubbyholes, that political behaviors, for instance, are not related to sociological behaviors. A favorite device of reviewers is to dismiss a book by saying: "This is all very well, but it isn't *psychology*, or *sociology*, or *political science*."

Katz and Schanck make no rigid distinctions between social and individual psychology. Such an approach "is an arbitrary division created by specialists, interested in different problems in the study of man" (p. 1). The first of four sections treats of the "social world of the man on the street." The authors speak of the ways common to people in an area, and of the ways considered atypical. They draw heavily upon the work of F. H. Allport (both received their degrees under him) in describing contemporary institutions. A particularly valuable part of their exposition is the constant reference to anthropological data, thus correcting the culture-blindness which has been a defect of much American social psychology. Particularly vivid is the discussion of revolution and the psychological factors leading to an overthrow. Deprivation does not necessarily lead to revolt; without leaders, propaganda, organization, ideology, etc., it can just as easily result in apathy and a crippling sense of isolation. The total situation must be ripe for a successful revolution. Even with all their institutional trappings and skillful pageantry, however, the Nazi leaders are "building on quicksand," because they have "a dynamic economic order to contend with" (p. 210), and their ordering of production will not settle problems of production and consumption.

The next two sections deal with "the scientific basis of social processes" and with "the social world of the clinician: personality." An attempt is made to describe the basic physiological and psychological mechanisms of behavior. The nervous and glandular systems are not described with any completeness—even the usual illustrations are omitted. A combined Gestalt-behavioristic position is adopted, with a generous sprinkling of Freud's and Adler's terminology. E. B. Holt's concept of adience behavior (tendency to continue in activity once started) is opposed to avoidance behavior. These two concepts are used to tie together quite dissimilar types of descriptions taken from the laboratory, the clinic, and the arm-chair.

The section on personality is disappointing. The authors try to apply the

Hegelian formula of thesis, antithesis, synthesis to "(1) the unification of personality, (2) the acquisition of social habits and attitudes, and (3) the growth of the self" (p. 475). In discussing the unification of personality, the authors hold that differentiation is "opposed" to integration, and that out of this conflict comes a "higher level" of conduct. The growth of the self involves the opposition between the pleasure principle and the reality principle. "The ego of the very young child is intensely selfish" (p. 476). But others "indoctrinate" the child with "habits and values of an unselfish sort" (p. 477), and the adult person emerges, having a "socialized ego in which short-sighted aggrandizement has become enlightened self-interest" (p. 477). To the present reviewer, this account seems an unnecessary and cumbersome addition to scientific description. Ordering terms are necessary, but Hegelian metaphysics adds little of value to our ways of talking about human beings. The authors do not even relate conflict to the opposition of muscle groups, as E. J. Kempf does. For them the thesis is a "system of factors in a definite set of relationships" (p. 475), and is thus highly abstracted from the meaningful behavior of people.

The authors pay their respects to Dewey's dictum that "we learn by doing." Yet, in common with the vast majority of writers of social science texts, they present nothing to do except passive reading. There are many class experiments possible dealing with rumor (they do not even discuss it), social facilitation, conflict situations and integration, effect of group discussion on individual opinions, stereotypes, etc. An inclusion of these would demonstrate to students the actual operation of some of the described psychological mechanisms. Without such observation, it is doubtful if most students have more than a vague word-understanding, which soon disappears after the course is finished.

The last section is a brave attempt to orient the student to culture changes. There is a long historical account of three phases of American history: the rural community, "the era of publics," and "the era of social classes." The Marxian outlook is adopted: economic determinism. Attitudes and ideologies arise from the economic relations of a given period. In the present era of social classes, the "situation is . . . exposed as one of a *struggle for power* by opposed groups of capital and labor" (p. 642, italics by the authors). The book closes with a call for people to formulate their values scientifically, that they may go out to act.

There are unfortunately many dogmatic and *a priori* statements in the book. Industrial workers "are classicists by temperament. Emotion *as a solution of problems* does not impress them" (p. 648, italics by the authors). "Science is a classical discipline, not a romantic one. It exalts collective experience over the single individual" (p. 687). "Only at the level of the psychologist can the problems [of social behavior] be clearly stated and the illogic exposed" (p. 557).

There is much emphasis upon logical dichotomies: voluntaristic is opposed to regimented behavior; society (which is primary) is different from culture (which is secondary); the Hegelian dialectic is utilized; conflict is distinguished from competition; simple is distinguished from generalized imitation; suggestion is opposed to trial and error learning; elementary identification is differentiated from social identification. For statistically-minded scientists, most of these oppositions are statements of the two ends of possible scales. As such they should be considered in terms of degrees, rather than in terms of either-or.

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CULTURE CONFLICT AND CRIME. A REPORT OF THE SUBCOMMITTEE ON DELINQUENCY OF THE COMMITTEE ON PERSONALITY AND CULTURE. By Thorsten Sellin. New York: Social Science Research Council, 1938. Pp. 116.

Professor Sellin undertakes in a scholarly manner to examine some of the current criminological research studies dealing with the so-called "conflict of cultures." In exploring this important segment of the broad field of crime causation he is seriously impressed with another conflict—a conflict in ideas. The lack of a standard terminology and the paucity of scientific knowledge regarding the causative processes in criminal behavior in general have bred, he believes, a confusion that must be cleared away before one can properly evaluate what has been done or suggest procedures for further research.

Some five years ago Michael and Adler in *Crime, law and social science* (N. Y.: Harcourt Brace, 1933) set out on a somewhat parallel course, reviewing the criminological literature in the field of causation, treatment and prevention. They too saw the necessity of clearing away the entangling underbrush with sharp and incisive logical tools. They emerged from the field with a feeling of despair. They saw practically nothing of scientific value. They went so far as to say that "an empirical science of criminology is not at present possible because no empirical sciences of psychology and sociology now exist. Criminological research . . . represents an attempt to do the impossible." The social sciences are separated from the physical sciences by the barrier of "free will." Voluntary action, they claimed, is unique and by its very nature is contingent and essentially unpredictable. Of all corporeal creatures man stands alone. Therefore, the argument ran, no necessary causal propositions can be made concerning human behavior. Michael and Adler admitted the possibility of some degree of accurate prediction, but on the whole, within this field of human behavior, they would limit generalizations to probabilities of varying degrees. Where conclusions can never be better than probable, they reasoned, our "science" is more like opinion than scientific truth. The best we could hope for would be the establishment of "probable generalizations . . . superior to existing opinion."

Social scientists have been slow to take up this challenge. They have been more concerned with producing socially useful knowledge than with making inquiries into the philosophical soundness of their assumptions. Professor Sellin in this report, probably because of his wide acquaintance with the foreign literature where such considerations are regarded as of more vital importance than they are in this country, has undertaken to meet this charge. He too examines some of the basic assumptions of research and finds them wanting. From that point on, however, the parallel fails to hold. He is ambitious and courageous enough to maintain that the rigid criteria of the natural sciences can be made to apply to the social sciences. He wants no double standard. He sees the need for a redefining of terms and the setting-up of a new frame of reference for criminological concepts. Only in this way, he insists, can research in social problems ever achieve the status of science.

The barriers raised by the critics he considers serious but not insurmountable. He admits that the intricate variables of human relationships do not readily allow clear-cut generalizations. He acknowledges the unique position of the social scientist who as observer is also a participant in the life he is investigating. But such special hazards, peculiar to the social sciences, should not, he insists, cause us to abandon hope.

Professor Sellin does not squarely meet the assertion of the critics that the uniqueness of human behavior, the essence of which is the operation of a

voluntary "uncaused" will, makes truth in the scientific sense now and forever completely unattainable for the social "scientist." Perhaps he does not wish to be drawn into the age-old "free will" controversy. He seems to skirt the problem by asserting simply that such behavior falls within the field of observable fact and therefore is relevant data for science. This contention, it seems to the reviewer, misses the point; for if the observable fact is not predetermined but, as Michael and Adler assert, indeterminate and "uncaused except by God," no laws or generalizations can be made. In other words, there will be no consistency in the facts observed, and without consistency no immutable laws are conceivable.

The author evidently is not interested in refuting this proposition. He prefers to fight it out on another field, probably feeling that if he is victorious there it will not be necessary for him to go back to the classical controversy. He urges the proposition of the indeterminacy of all things. Though this argument is getting to be an old story, his vigorous defence of it will be a comfort to all social scientists who feel inferior in the presence of the physicists, the chemists and the astronomers. Mathematics, he points out, is the only science where absolute prediction is possible; and this is so, of course, because of its characteristic method — reaching logical conclusions from assumed hypotheses. All other sciences, physics as well as sociology and psychology, state their laws only in terms of probability; all of the data are merely contingent. "The physicist penetrating the interior of the atom, is equally unable to predict the dynamic movements of each infinitesimal particle in its universe and this in part has led to that swing toward a belief in the principle of indeterminacy, which has been stated so frequently in recent years by the physicist." What seems to be, therefore, the exclusive defect in sociology and psychology as sciences, is common to all of the physical sciences, though perhaps in a less degree.

Professor Sellin's manner of reasoning is more convincing than that of the early behaviorists in psychology who refused to admit of anything less than strict determinism in either physics or psychology. It does not follow, however, that this new law of indeterminacy now in popular favor among physicists, is going to make it any easier to establish the social sciences on a firmer—that is, more scientific—basis. Instead of straining to reach the moon (as the behaviorists did), or conceding the impossibility of this feat (as Michael and Adler do), the author would bring the moon a little nearer the earth, believing that in time there may be no impassable distance between them. At any rate, it seems to the reviewer that the difference between the best status that Michael and Adler would allow to psychology and sociology, namely "probable generalizations better than opinion or common sense," and the ideal set by the new physics that is now presumably loosing its hold on the precious doctrine of strict determinancy, is not great enough to make any practical difference to criminological research. The best safeguard is the use of appropriate scientific methods.

In the belief that criminology can attain the status of a science Professor Sellin goes on to examine some of the procedures of investigation and to inquire into the adequacy of the basic concepts now underlying research in crime causation. As to the former, he finds as Michael and Adler and others have pointed out, a serious disregard for the necessity of formulating the theoretical problems behind the research. As for the scientific nature of the concepts used, he finds confusion at the very core. The limits of the field of study vary from one authority to the next. There is no agreement

over "*the* crucial problem in criminology"—the definition of "crime" and "criminal." The question, who is a criminal and who is not, has been variously answered. Many writers have preferred the simpler course and are in agreement with Michael and Adler in the view that "not only is the legal definition of crime precise and unambiguous, but it is the *only possible* definition of crime." Professor Sellin, on the other hand, is impressed with the great variability of the criminal law in time and space and with the fact that the law is often shaped by minority groups leading to "a lack of congruence between the laws of a state and moral ideas of different social groups within its population." From the author's picture the reader must conclude that criminology as it now stands cannot possibly grow up into a science without a very radical revision of its basic concepts.

Professor Sellin meets the need of supplying universal categories by suggesting the adoption of "conduct norms" to transcend criminal or legal norms. They satisfy the need for universality, for they are found wherever social groups are found. They can be described as the attitudes of the group toward the conduct of its individual members—attitudes that have become crystallized into rules. They are, of course, incorporated within the personalities of the individuals of the group and can be examined by studying either the individual or the violations in the groups or areas where conflicts between norms occur. Whatever one does that runs counter to group opinion and arouses a disapproving attitude, whether or not it is prescribed by law, comes within the field of study. What, one may ask, has this to do with the chosen problems of the criminologist? All causative studies in criminology, Professor Sellin would answer, are focused on the conflict of one cultural norm with another. He quotes E. H. Sutherland, his colleague on the Subcommittee on Delinquency, to the effect that "the conflict of cultures is . . . the fundamental principle in the explanation of crime." He sees the criminologist, now confined to a specialized interest in the criminal norm, eventually extending that interest to include a broad "ethological" field.

One is immediately aware of the hazards involved in the establishment of these norms. How are these norms to be identified and described in a complex culture? By what standards are they to be measured and classified? These difficulties do not escape the attention of the author; in fact, he makes a number of suggestions for the classification of norms and for appropriate procedures for studying them. He sees no hope at all for the "science" without broad and universal categories such as he has proposed. The reviewer wonders if it is any less scientific to take as criterion the narrower criminal norm where the limitations are known and acknowledged but where the content is specific and clear, than it is to adopt the concept of the cultural norm where universality is achieved at the expense of clear-cut definition. The author presumably believes that the separation, identification and measurement of these norms even in our complicated society is not too great a problem.

Criminological research studies dealing with the "culture conflict" have dealt largely with the immigrant groups who have come into conflict with legal norms of the areas to which they have migrated. Professor Sellin reviews this literature. He again finds a number of stumbling blocks to good research, such as the mobility of the migrant groups, the confusion between nativity and race, the lack of sound criminal statistics, and so on. No clear picture of the causative elements in criminal conduct emerges. Against this rather barren background the author sets down some constructive suggestions. He

concludes by offering to the research student a great number of original and stimulating research problems within the frame of the "culture conflict."

In reading this report one cannot escape the conviction that the author has brought to bear some original and vigorous thinking on the old problems of research in the social sciences. One looks in vain, however, for any statement of *how* the suggested studies will illuminate (beyond mere description) the essential causes of crime. In extending the field of research to embrace the larger categories will it not be necessary to draw upon some or all of the other social sciences? How, for instance, are we to describe and identify conduct norms without turning to the normative science of ethics? How are we to know something of the origins and natures of various cultures without drawing upon the resources of the anthropologists and sociologists? How are we to understand the relationship between delinquency and the culture conflict or the essential nature of the conflict itself as expressed by the individual, without a knowledge of clinical psychology or psychiatry? How are we to investigate the problem of social attitudes (as reflected in the cultural norms) without the psychologist's contribution to the study of attitudes as such? Professor Sellin has not made clear, perhaps he has not sufficiently recognized, the interrelationships of the social sciences within his research program. The main weakness of a research program that seeks, as this one does, to get at fundamental causes is its failure to see the need of going below the surface conflicts to explore the dynamic interplay of forces within the personality.

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WHAT MAN HAS MADE OF MAN. A STUDY OF THE CONSEQUENCES OF PLATONISM AND POSITIVISM IN PSYCHOLOGY. By Mortimer J. Adler. (With an Introduction by Dr. Franz Alexander.) New York: Longmans, Green and Co., 1937. Pp. xix+246.

This interesting and provocative volume may well be presented as a literary event. What reader could withhold his interest from a Thomistic treatment of psychology written by one who for six years taught experimental psychology at Columbia University? Or from a book whose introducer offers it as a classic example of turning back the clock of history and science? Again, who could fail to be provoked by a volume which, though concerned with the same materials as scientific and experimental psychology, reaches the conclusion that Aquinas is still right, against the whole galaxy of laboratory workers?

A description of the book should begin with the recital of its origin. The director of the Psychoanalytic Institute of Chicago, believing that persons engaged in technical pursuits are apt to become intellectually restricted, hit upon the idea of giving his students and staff an opportunity to enlarge their intellectual horizon. Accordingly, he invited Dr. Adler of the University of Chicago to regale them with a course of four lectures on epistemology and methodology. In printed form the lectures are entitled: (1) The conception of science in the modern world, (2) The position of psychology in philosophy and among the natural sciences, (3) The history of psychology, and (4) Psychoanalysis as psychology. To these lectures, arranged as hierarchical propositions and sub-propositions covering about 120 pages, have been added 120 pages of notes in regularly set format. The content is indicated in the following theses.

Lecture I. (1) There are two kinds of knowledge: (a) sense-observational-

perceptions, abilities, exercised through sense-organs and brain; (b) intellect-reflective—namely, abstraction, judgment and reasoning. (2) The proper object of: (a) sense is the thing as particular physical object, (b) intellect, the thing as universal—for example, non-physical things, souls, angels, God. (3)(a) Science deals with the phenomenal, the descriptive, (b) philosophy with the noumenal, the explanatory. (4) Psychology more than any other science is, on the one hand, investigative and, on the other, philosophical.

Lecture II. (1) Primarily intellectual operations are not acts of a bodily organ. (2)(a) Man differs essentially and specifically from all other animals; (b) evolution and biological continuity are folklore. (3) Psychological knowledge is possible without investigation.

Lecture III. (1) Up to the middle of the 17th century nothing but error had been added to the psychology perfected by St. Thomas Aquinas. (2) But since that time truly scientific psychology has been fostered by psychoanalysts and psychometricians because they, like St. Thomas, investigate man as man with his unique faculties and powers and not man as animal.

Lecture IV. (1) Psychoanalysis, however, falls short because it does not induce sound moral principles, so that persons can direct their lives by the rule of reason instead of merely relieving immediate difficulties. (2) Psychoanalysts commit the error of not recognizing that moral principles are based upon speculative truths that hold for all men and are not relative and changing.

What one can make of this Thomistic treatment of psychology is of three sorts. First, one can cavalierly dismiss it as unworthy of time and attention. Such a gesture of impatience, however, will hardly suffice, inasmuch as the author has cunningly revealed the common ground between modern mentalistic and scholastic psychology. Can those psychologists who deal with factors, abilities, unique personalities and disguised faculties effectually divorce themselves from the soul psychology of St. Thomas? Perhaps it is unfortunate for Adler's opponents that he knows enough neurology and physiology to realize that the latter provide no means of concealing the traffic with spirits. Possibly nothing more than scientific candor is required to admit Adler's accusation that the non-Thomistic psychologists merely commit the error of pride in not recognizing themselves as the inheritors and continuators of the European (Scholastic) tradition.

On the other hand, one may acknowledge that non-Thomistic psychology, whether psychoanalytic or of some other system, rests upon a common psychic foundation with the Thomistic and may ask whether in that case either superstructure can claim superiority. How can one meet Adler's contention that despite the emphasis upon laboratory investigations by the non-Thomistic psychologists, their preoccupation with states of consciousness which cannot be investigated makes psychology merely physiology or animal psychology on the one side and philosophy on the other? And what of the implication that the study of animal behavior, sensory processes and reaction time leaves the fundamental problems of human psychology hanging in the balance? If Adler's criticisms do touch crucial questions, then in spite of his Thomistic faith the effect on the part of those he criticizes is doubt and hesitation.

Then there is a third way to handle the material contained in this volume—namely, to take a stand upon observed events and ask why both sorts of philosophical faith cannot be extruded from psychology. The present volume serves as an admirable aid for this purpose, since Adler's contentions against psychoanalytic and other mentalistic psychology turn out to be instruments of self-destruction. If modern psychology is full of schools and has made

little progress because it is a continuation of Scholastic philosophy and religion, does that not indeed condemn medieval dialectics? If laboratory psychologists have mixed dialectics with their observational techniques, it is now indicated that they should dispose of the dialectics.

From the standpoint of this third attitude, then, there is good reason for carefully examining a few of Adler's basic theses. Turn to the proposition, first, that, because hypotheses and postulates in addition to laboratory manipulation, are essential for all scientific work, psychology is therefore partially philosophical. Grant him the boon of calling scientific logic and reasoning philosophy. But does this mean an inevitable leap into the arms of St. Thomas? Unless we heed the voice of religious authority and deafen ourselves to the insistent call of facts, we can find ample enough scientific logic derived from the accumulated interbehavior with verified results to suffice for all the needs of psychology as well as the other sciences. On this basis, moreover, we can save ourselves from the magic ring of verbalistic philosophy which merges with a worse psychology and again reappears as the same indefensible philosophy.

Glance now at a subordinate and supporting thesis—namely, the essential difference between sensory and intellectual knowledge. The former, Adler agrees with his opponents, constitutes actions of organs or organisms and therefore is common to men and animals. But there is no organ for intellectual action—abstraction, judgment, reasoning. Hence that action, he contends, establishes the existence of the soul.

This argument reveals a clever handling of conventional psychology. Adler is trading upon the mentalistic psychologist's attempt to relate sensory phenomena with specific neural functions. Whether or not these psychologists have ever really asserted that the brain, for example, is an organ of sensory knowledge in any other way than it is for reasoning—namely, that all mental phenomena are connected with the actions of organisms—conventional psychology does allow for non-organic or psychic powers. Now since such non-organic (non-observable) processes are fair game for all poachers, Adler may just as well adopt his soul faith as his brother beneath the skin embraces that of discrete psychic processes.

Since it is axiomatic that faith is autistic and cannot be argued, the obvious implication is to abjure both forms. Furthermore, as soon as observed phenomena are given decisive weight, then it is clear that the post-Reformation psychic process and the pre-Reformation soul both are names for the interaction of organisms with stimulus objects. In sensory discrimination the organism interbehaves with objects in direct contact and in reasoning with things substituted for. It is such direct objects that Adler calls individual, while those substituted for and existing only in the symbol constitute his universals. Adler's thinking is patent here. In true Scholastic fashion he transforms verbal formulae, definitions, and accumulated knowledge into pure intuitional acts, or soul actions, and into supersensuous objects.

Because we are interested in the bearing this book has upon conventional psychology and because Adler's copious appeals to fact illustrate how they can be marshalled in the interest of preconstructed faith, we exhibit his proof of the absolute difference between man and the other animals. Here he gathers together a set of differences—for example, (1) animals have only sensitive and not reasoning knowledge, (2) animal societies are instinctive (biological) not moral and volitional, (3) animals can make things, but not vary or multiply them, (4) animals have no language, and finally (5) they

cannot be psychoanalyzed. We may point out three specific techniques of demonstration. First, Adler creates his own facts by definition. By asserting that language, reasoning and analysis are human, he absolutely sets off the human from other animals in the interest of his Scholastic prejudices. At times, however, he goes so far as to say that behavioristic studies of problem-solving are investigations of rational behavior, though only the "accidents" of such behavior. Again, he falsifies facts by attributing properties to human beings which they do not have and by robbing animals of ways of behaving that they constantly exhibit. When facts contradict his Reason, it is just too bad about the facts. And finally, he isolates such differences as can be found among animals at any given point in the phylogenetic scale and hypostatizes them as absolutes, essentialities.

As to the other theses—for example, that psychology and psychotherapeutics should fit into a system of Reason and absolute moral principles, we remain silent in view of the author's complete escape from the scene of human action. But we cannot forbear to mention how full of inconsistencies and contradictions is the work of such an apostle of absolute logic and Reason. We quote one example.

> In general, it is a misfortune to be born in modern times, a period in which each thinker commits the sin of pride, acknowledges no ancestors, rests in no tradition, but insists with consummate egotism, upon his complete originality and his complete independence of all prior human learning (68).

And yet he presumes to correct the Angelic and Universal Doctor (80).

In finally appraising this volume, however, the reviewer believes that it must not be judged merely as an argument for the revival of medieval thought, nor as a tissue of fundamental contradictions, nor yet as a deceiving plea for human welfare, but rather as a choice example of the *reductio ad absurdum* of many of the doctrines of modern psychology. It is in this sense that it deserves careful reading by students who value intellectual orientation.

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REDISCOVERING THE ADOLESCENT. By H. Dimock. New York: The Association Press, 1938. Pp. xx+287.

In recent years, the adolescent ages have again occupied the attention of investigators so long engrossed with early childhood. The techniques employed, however, are often inadequate, and the conclusions reached are frequently of questionable validity. Most investigators, it appears, continue to make cross-sectional studies of different children at various age levels. This methodology frequently obscures those significant aspects of development which can be discerned only by attention to the *process* of growth over an extended period of time. Dr. Dimock's volume, *Rediscovering the adolescent*, is of the latter type; in it the development of 200 boys during several years is described. This period carries a majority of the boys from the prepubescent through the postpubescent stage. Dr. Dimock brings together a large amount of varied but related data from many approaches. The result is a rather clear picture of the progress of the typical boy during a critical period of life and growth.

Answers were sought to questions concerning: the age at which normal boys enter pubescence; factors associated with early pubescence; the nature and sequence of motor development; favorite play activities; the distribution of time; the factors fostering friendship and group acceptability and a host of significant related problems.

Space permits comments on a few of the results only. The records of the way time is spent corroborate some of the data reported in *Youth tells their story* and other accounts of the interests and activities of present-day youth. Activities in which the maximum decrease in time allotment occurred during the investigation included the following in this order: "sleep; participation in physical play; eating; club activities; reading; miscellaneous activities such as playing music or following hobbies; amusements outside the home; and doing chores." One finds that there takes place, as maturity approaches, a transition toward the stereotyped activity of the typical adult; standardized and restricting influences appear to be very active and potent during these years.

It is significant, too, that as these boys become older, they spend an increasing amount of time as *spectators* of games and sports. Indeed, for twenty-five per cent of the boys, participation in these activities is negligible. The "bleacher athlete" is being developed. It is distressing to find that participation in club activities also decreases during this period, and that with an increase in amount of time spent in "study for school" opportunities for the intensification and expansion of worthy individual and group interests are crowded out.

A similar curtailment of *spontaneity* is revealed in many sections of this book. For example, Dr. Dimock's painstaking study of play activities demonstrated conclusively that the number of play activities participated in decreases with increase in age. Thus, as the boy becomes older, he narrows his range of activities, spending larger blocks of his time in fewer activities. In this connection, the following is significant. The "big four" activities in the passive-spectator category include listening to the radio, going to the movies, riding in an auto, and "watching athletic sports." These and other passive or amusement types overshadow—indeed, replace—active and creative pursuits. Educators, psychologists, and community leaders may profitably ask: how may this period of growth be safeguarded from the domination of ready-made brands of amusement proffered by the universally appealing methods of shrewd promoters? And how may interested persons work together so as to encourage the development of more worthy and hygienically sound patterns of activity which will carry through to wholesome adulthood?

By use of a rating device, Dr. Dimock attempted to ascertain the amount and nature of personality disorder or maladjustment among these boys. Of the 200 boys, twenty-seven per cent were described by this method as maladjusted. Many of these fifty-five boys, Dr. Dimock reports, require the services of a psychiatrist. They have, however, an additional need. All of these young people and most of the others appear to the reviewer to need, not only these special services, but also a different and richer environment in which basic drives and worthy interests may be expressed. It is to be hoped that the reader of this chapter will view these facts of *maladjustment* with full recognition of the social situation which has had so important a part in creating them. Not only do we find that a restricted and stereotyped environment contributes to the production of unusually large numbers of maladjusted young people, but we discover also in the chapters devoted to social acceptability that "perhaps 15 to 25 per cent of the boys, possess an acceptability status that we have arbitrarily judged to be below the minimum needs for wholesome and satisfying personality." Friendships appear to be casually and fortuitously developed, for the social setting is barren of experiences which lead to common concern and purpose basic to enduring friendships.

The static life of these young people during this crucial period is portrayed

in many sections of the book; for example, in the stability of certain play and recreation patterns and in the nature of religious experience and thought. The sterile or barren moral and religious concepts which are maintained over a four-year period will be disconcerting for those persons to view who believe that these thought forms should be progressively organized and developed to conform with the changing demands for understanding and interpreting a world in transition and chaos.

Another bit of conduct and behavior data adds consistency to the foregoing picture: "When the factors associated with consistency of conduct were investigated, it was discovered that consistency, when present, was a function of group life and experience. If, as adults, we desire to see some degree of integration and consistency in the conduct of children, we must recognize and utilize the unifying concept of cohesive groups." Thus, fragment by fragment, the presentation takes a total form which leads to one inescapable conclusion: our contemporary society is overburdened with obstacles to wholesome personality orientation and growth. Our social fabric contains numerous agencies and institutions, which, in their present forms, are far removed from the personal and social needs of adolescents today. These agencies are shown to affect and standardize the life and thought of the home, the school, and the community. The task of social reconstruction and individual rehabilitation, therefore, rests upon a concerted effort to foster sturdy growth from childhood to adequate adulthood through the creation of new and more appropriate designs for living. Dr. Dimock's excellent study reveals these needs and suggests the direction which an attack on the problem may well take.

One should not leave this fine contribution without comment upon the steadfastness and thoroughness with which the author investigated these problems. His scientific acumen is equalled by his objectivity and impartiality in interpreting the data. Thus, the book provides a praiseworthy model which many workers who are vaguely exploring adolescence may profitably examine. The pictographs of Harold Haydon vividly portray the statistical data, and add measurably to this volume which should be read by all students of child growth in contemporary America.

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PSYCHOLOGY AND THE MOTORIST. By Herbert A. Toops and S. Edson Haven. Columbus, Ohio: R. G. Adams and Co., 1938. Pp. vi+265.

Psychologists are probably as persistent conversationalists about their driving and their adventures on the highway as any other people. But their professional attention to the problems of driving skill and highway safety has been almost negligible. Toops and Haven must be credited with an effort to remedy this neglect. *Psychology and the motorist* is at least a general book about driving written by psychologists. Other psychologists should read it if for no other reason than to note the opportunities for work in a field which is at present the happy hunting ground of highway engineers, safety propagandists, bewildered police officials, and politicians.

Certain advantages of a psychological approach to the field are immediately evident in this book. The moralistic tone of the traffic court and the safety pamphlet gives way to a detached consideration of the behavior of drivers, their habits, and the restrictions under which they drive. The effectiveness for behavior of things like laws, highway markers, speed limits, and four-lane

highways can then be treated as a psychological problem. So-called causes of accidents like recklessness and speeding are seen to be psychologically worthless, inasmuch as they are condemnations rather than explanations of the behavior in question. The puritanical proposals of many safety reformers can be criticised on purely practical grounds; there is the psychological possibility that a motorist bombarded by fear-propaganda is less apt to be "frightened into caution" than he is to be "scared into ineptitude." The book, in short, takes the point of view of the driver as an individual, rather than that of the host of professionals who are at present engaged in restraining him without understanding him. It is based in part on interviews with drivers made by the authors' students.

But although *Psychology and the motorist* opens up a field with stimulating possibilities, it will be a disappointment to many psychological readers on the one hand and will probably not convince the traffic engineers on the other. The psychology is too superficial for the former, and the recommendations are too amateurish for the latter. Essentially it is a series of more or less novel proposals for safer and more pleasant driving, some of them as fanciful as the suggestion that a compartment in the engine-block be provided for boiling coffee on the run. There is no account of past psychological investigations, for what they are worth, and scarcely a reference to any source of information. The earlier work at Ohio State University and the psychological tests for driving-skill developed by DeSilva and Lauer are not mentioned. Such clinical tests for drivers are dismissed by implication in a paragraph on the meaninglessness of measuring a driver's reaction-time. A few anecdotal pages on space-perception, which is properly a central problem for the psychology of locomotion, betray the assumption that the perception of distance and speed depends on "inference" and that therefore it is no wonder that drivers have accidents.

A thoroughgoing analysis of the behavior of a man with an automobile would have to include an examination of the nature of locomotion, of destination and path, of obstacles and barriers, of perceptual constancy, and a number of other such abstract problems. Books sometimes have the useful function of arousing other writers out of their lethargy in the conviction that a better book can be written on the same subject. It is to be hoped that this is a book of that sort.

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THE KELLEY STATISTICAL TABLES. By Truman L. Kelley. New York: The Macmillan Company, 1938. Pp. v+136.

For many studies, especially some involving item analysis, there has been a need for a more adequate normal probability table with the argument or value for entering the table in terms of area or proportion (p), rather than in terms of the abscissa or distance measured from the mean of the normal distribution.

In the present volume, Kelley presents an eight-place table of the normal probability function. This table has the argument p from 0.5000 to 0.9999, and the corresponding complementary values of q from 0.5000 to 0.0001. The normal probability functions, x and z (x is the distance from the mean to the point of dichotomy, and z is the ordinate at the point), are given to eight

places of decimals. In addition, the table contains the entries $\sqrt{pq}$, $\sqrt{1-p}$, and $\sqrt{1-q}$. These are useful in a variety of computations in probability and correlation functions.

Much care was taken in the computation and checking of the values of this table so that the entries are accurate to the last published figure. In some studies, however, it is necessary to use the abscissa as the argument and the area (p) as the table entry. In such cases it is often necessary to use interpolation to obtain a sufficiently accurate value. To make such interpolation easier, Tables III, IV, and V were constructed. These tables give cubic, quintic and septic interpolation coefficients, making possible as accurate interpolation as may be needed. These coefficients may be used in interpolation in a variety of mathematical tables where the entry is by means of equally spaced arguments.

There are three other tables in the book. Table II is a four-place table of chi-square functions, with an argument of χ^2/n (where n indicates the degrees of freedom) with table values of P, the probability that, for a given n a divergence as great as χ^2 will arise as a matter of chance. Table VI gives the square roots of numbers 1 to 1000 to eight digits. Table VII gives the values of a number of constants such as 2, ½, $\sqrt{2}$, π, e, to twelve places of decimals, and their common logarithms to fifteen places of decimals.

The various tables, auxiliary to the eight-place table of the unit normal probability distribution, add considerably to the value of the book. The tables are intended for statisticians whose work demands a considerable degree of accuracy and refinement.

Because of excellent choice of type, style, and size, and the adequate size of the pages, the tables are exceptionally easy to read. One should expect a minimum of error to creep into his computations as a result of misreading the tabled values. It is one of the best jobs of typography that the reviewer has seen.

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NEW BOOKS RECEIVED

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